

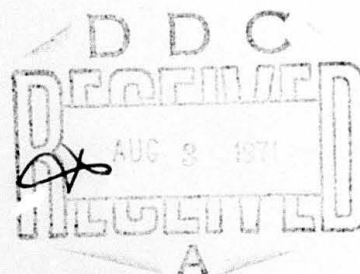
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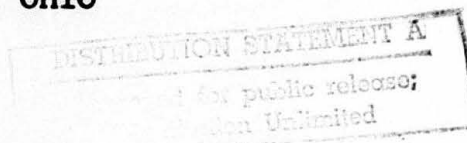
**RESEARCH AND DEVELOPMENT**  
**A CATEGORIZATION OF  
THE METHODS AND TECHNIQUES OF  
MEASURING INDUSTRIAL R & D ACTIVITIES**

By

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## Preface

For many years I have been closely associated with both the scientific and the managerial aspects of research and development programs. Within my Air Force career field, I have held fully qualified specialty codes as a "Developmental Engineer for Aerospace Systems" and as a "Research and Development Officer." I was a member of the Weapons System Phasing Group (WS-117L) which originally planned many of our present Air Force space programs. Later, while assigned to the Office of the Secretary of the Air Force, I served as a project officer on various space programs with the OSAF's Special Projects Office and subsequently with the Air Force Systems Command's Space Systems Division. I was a payload engineer, a booster systems engineer, and the officer in charge of the milestone and scheduling program for our nation's top-priority space satellite system. And, I was the PERT Officer for the first Air Force space project employing this management and control technique and was the Configuration Management Officer for the space vehicle most widely used for USAF and NASA space programs.

While serving in these capacities, I became concerned with the methods of evaluating the effectiveness of the research and development efforts being conducted by and for the Air Force -- where the only measurements of performance in such non-profit oriented programs appeared to be the amount of variance from targeted schedules and costs and the degree of success exhibited in achieving project goals when the "bird" was finally flown. For these reasons, I began to review the literature regarding the measurement techniques employed within industry to evaluate research and development efforts -- thinking that in such a profit-motivated environment, more concrete means of analyzing effectiveness might have been developed.

After compiling these evaluation methods for a number of years, I thought that others might profit from my own research of the applicable literature and I, therefore, wrote a very brief report of my findings, entitled "Research and Development: Appraisal and Evaluation." Subsequently published by the Air Force Institute of Technology's School of Systems and Logistics in November 1970 and disseminated through the Defense Documentation Center, the report quickly gained widespread acceptance -- with requests for its use being received from many different agencies, including:

(1) the U.S. Atomic Energy Commission, as well as the Sandia Laboratories, sponsored by the Commission; (2) Department of Defense organizations such as the Institute of Defense Analyses, in conjunction with studies for the DOD's Weapons System Evaluation Group; the Air Force Technical Applications Center of Headquarters, USAF; as well as the Air Force's Office of Scientific Research, and the Air Force Systems Command's Aeronautical Systems Division, Aerospace Medical Division, and Aerospace Research Laboratories; (3) civilian agencies such as the Bell Telephone Laboratories, sponsored by the U.S.A. Safeguard Systems Command; Lockheed-Georgia Company, sponsored by Headquarters, Aeronautical Systems Division; and Aerojet General Corporation, sponsored by the U.S. Army's San Francisco Procurement Agency; and (4) departments of management within academic institutions such as Indiana University, Ohio State University, Wright-State University, and many other schools.

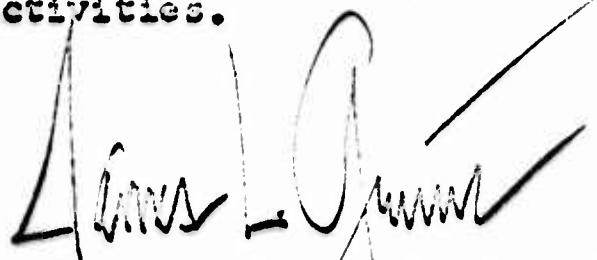
The very broad interest in the report was typified by the statement of one requester, who said, "A document that coherently summarizes appraisal and evaluation techniques could be of significant value if used in a conscientiously applied program of research evaluation and regular professional review." The highly favorable comments received after publication of the initial report prompted me to prepare a more detailed documentation of my findings regarding the methods and techniques of evaluating and measuring research and development efforts. Thus, this expanded report is being published to meet the apparent needs of many organizations interested in the management of scientific and technical activities.

This report, like the previous one, indicates that the evaluation of scientific performance in the industrial environment is beset with many of the same problems that I had noted in the non-profit oriented military climate. Apparently, no matter what the R&D environment, the general rule for appraising the potential of proposed research, measuring the effectiveness of in-progress activities, and evaluating the results obtained from these efforts is simply the following: "Use quantitative methods, wherever appropriate, to assist in making qualitative judgments."

Although literally hundreds of reference sources were reviewed in preparing this report, I found several of especial interest and value, as may be noted in referring to my footnotes citing these sources. Particularly meaningful inputs were obtained from the extensive material authored by James B. Quinn, as well as by Raymond Villers

and Walter McFarland. I also found a wealth of material included in the book on administering research and development compiled by Charles Orth, Joseph Bailey, and Francis Wolek, and in the handbook on managing industrial research edited by Carl Heyel. And, James Bright's books on technological innovation were very informative as was Daniel Roman's text on management of research and development. Indeed, should any of my readers wish to expand their knowledge of the methods and techniques of evaluating research and development efforts, any of these cited authors along with many of those referenced in this report would be excellent sources of information relating to the management of scientific and technological activities.

February 1971  
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## Abstract

Rapidly advancing technology and accelerating scientific progress have created a dynamic and constantly changing industrial environment necessitating ever greater emphasis on the research and development function within all organizations. The measurement and evaluation of the R&D function is difficult but nonetheless necessary, and many different methods and techniques have been devised and used to predict and determine the results achieved from scientific and technological endeavor.

Ranging across the quantitative to the qualitative spectrum of methodology, many various means are employed to appraise, measure, and evaluate research efforts and developmental activities. Variables which affect the selection of a particular method for analysis include such considerations as the type of research agency involved, the level of the research effort, and the stage in the developmental life-cycle of the research to be evaluated. A method which might be quite appropriate under one set of variables might be completely inapplicable under another set of conditions.

This document reports many of these means of analysis actually employed in measuring and evaluating research and development, and it classifies these methods and techniques as to their applicability for pre-appraisal, in-process measurement, and post-evaluation. The report concludes that, although the quantitative and qualitative methods are at the opposite ends of the measurement spectrum, a successful R&D manager will use quantitative means, whenever appropriate, to assist him in making qualitative judgments and decisions.

## RESEARCH AND DEVELOPMENT

### A CATEGORIZATION OF THE METHODS AND TECHNIQUES OF MEASURING INDUSTRIAL R & D ACTIVITIES

#### I. Introduction

Ours has become a dynamic and changing society. Rapid technological advances and new scientific discoveries have changed the face of our economy. New products, markets, and demands have altered the structure of our industrial complex. Within this dynamic pattern, more and more companies and industries have come to realize that their place in this structure depends upon their adequately meeting the changing requirements. To improve, indeed even to maintain, their relative position within the economy necessitates an emphasis on the technological aspects of their operations. Thus, increasingly greater attention is being focused upon the research function of their organizations and the results which may be obtained from industrial research and development.

## II. Evaluation of Research Activities

Industrial research activities have become of ever-increasing importance to the vitality and growth of our economy. One authority has stated, "Many companies and many industries realize today that technology is an important raw material in their operations. . . . The effectiveness of industrial research can affect a company's competitive position, its future growth, its employee relations, and almost everything else connected with what the company does."<sup>1</sup> Another has said, "Today's research determines tomorrow's growth. Industry position, profits, and research are inseparable in long term planning for corporate growth. . . . This industrial growth and development is based upon research."<sup>2</sup>

With these facts in mind, industry has given increasingly greater support to research efforts. Almost all major corporations and many industry associations have established research facilities of unprecedented magnitude. Industry has funded much of the research conducted by private research organizations and academic institutions and has established special facilities to evaluate the potential commercial applications of the "spin-off" from various research conducted and sponsored by the government. "Research has become a mighty industry in its own right -- perhaps the key industry of our economy."<sup>3</sup>

Huge sums are being spent annually on research endeavor. Compared to the total research expenditures of little more than \$100 million in 1928 and only \$300 million per year immediately prior to the Second World War,



research expenditures in the decade of the 1960's exceeded a rate of \$12 billion annually -- approximately two per cent of our gross national product<sup>4</sup> -- and they should be substantially greater in our present decade. Although more than half these expenditures are funded by the Federal Government, funds contributed by industry amount to almost two per cent of the net sales realized by companies performing research and development.<sup>5</sup> Estimates have been made that technological advances resulting from research endeavor account for 90 per cent of the impressive increase in United States productivity or output per man-hour while only 10 per cent is attributable to increases in capital formation.<sup>6</sup>

Raymond Ewell of the National Science Foundation has estimated that the economy has earned a yield of from 100 to 200 per cent a year from its investment in research over the past quarter of a century;<sup>7</sup> James B. Quinn of Dartmouth's Tuck School of Business Administration pointed out that our annual expenditures on research far exceed the total amount spent from the birth of the nation to the Great Depression;<sup>8</sup> and W. D. Seyfried, Manager of Research for Humble Oil and Refining Company, stated that if the present trend should continue, spending over the next five years could be greater than \$100 billion.<sup>9</sup> The latter prediction, made in 1963, has been borne out by the evidence of our times.

Although some people discount the totals now being expended as being overstated because of inflationary research costs and broadening definitions of research classifications, others contend that the expenditures are in fact deflated because of the greatly increased scientific

productivity which has resulted from the use of more highly qualified research personnel and of greatly improved test equipment and modern computers.<sup>10</sup> Regardless of which is true, the increase in outlays for research and development has been tremendous and the rate of expenditures will continue to accelerate at an even greater pace in the years to come.

### Difficulty of Evaluating Performance

The vast amount of money being spent annually on research efforts emphasizes one of the most important problems inherent to research organizations -- that of determining some means of evaluating or measuring the performance of the research function. Robert Tannebaum of the University of California's Institute of Industrial Relations described the problem as follows:<sup>11</sup>

Today, millions of dollars are being spent on research and development work. Individuals, isolated research teams, and large-scale laboratories are working toward the development of new ideas and methods. When such large sums of money and numbers of people are concerned, the sound formulation of objectives and careful evaluation of progress toward these objectives become of increasing importance. However, the formulation of objectives and the evaluation of performance in the research and development field have involved so many intangibles that the evolution of useful guidelines has proven to be most difficult.

This position is substantiated by Edward B. Roberts, in his book, The Dynamics of Research and Development:<sup>12</sup>

These intangible, imprecise, and uncertain factors prevent charting the accomplishment of a significant research and development undertaking.

. . . Attempts to apply standard control and evaluation techniques to the research program have proved very disappointing. . . . The problems involving measuring and responding to progress in a research and development project . . . arise out of the inapplicability to research and development of existing control methods. . . . Project managers continuously try to state what progress has been made, but no necessarily relevant measurements are available to verify or deny such statements.

This appears to be the crux of the problem. "By its very nature, research is the business of getting disorganized."<sup>13</sup> Because of this, the research function (and the effects achieved therefrom) tends to defy measurement. The imperspicuous and chimerical nature of measuring research performance is cited in virtual unanimity by all authorities on the subject.

Herbert A. Shepard, in describing the dilemmas of industrial research and the inapplicability of traditional methods of measurement, stated:<sup>14</sup>

Probably no other branch of industrial activity causes so much managerial unrest and uncertainty as does research and development. The concern is due largely to the enormous difficulty of evaluating the laboratory's actual and potential economic contributions to the firm.

In a study for the National Industrial Conference Board, Nestor Terleckyj and Harriet Halper found that:<sup>15</sup>

Although some limited information exists suggesting that the relationship between research and development, on the one hand, and growth, productivity, and profits of industries and companies, on the other, is positive, no quantitative measures of R&D payoff have as yet been developed.

In view of the diversity of R&D operations and the lack of any identifiable product unit, it should be clear that no general measure of output of R&D can be devised at present.

In discussing the general uncertainty of the incidence of exploitable research findings, Ralph E. Burgess, a leading consultant, noted, "This vagueness makes it difficult to measure the gains from past research, to determine the proper scope of current effort, to establish a cut-off point for projects that appear fruitless, and to anticipate future gains."<sup>16</sup> After a detailed study of management controls in industrial research organizations, Robert N. Anthony concluded, "There seems no way of measuring quantitatively the performance of a research laboratory. Competent persons are generally reluctant to rate the performance of various laboratories on any basis."<sup>17</sup> His findings were further substantiated by the results of a National Science Foundation survey which found that less than a quarter of 200 companies interviewed reported any formal methods of estimating research results.<sup>18</sup>

Typically, research results are difficult to predict, projections of intended results are usually of insufficient accuracy to serve as standards of performance, and actual results are frequently quite different from those planned. Often the only results obtained from many research programs are an increased knowledge of the subject area, the formulation of new ideas, and an improvement of research techniques -- all admittedly of an intangible nature. Further, the fruits of research may not be realized until some distant future date. Because the particular application of some given research results may lie dormant for decades, the full economic impact and return on investment may not be realized until many years after



the research program was performed. Summing up these problems, James B. Quinn said, "Comparing intangible results with inaccurate forecasts can only yield nebulous conclusions."<sup>19</sup>

### Necessity for Measuring Effectiveness

Although there is almost universal agreement as to the impossibility of measuring or evaluating the research function, all authorities on the subject also agree as to the necessity of doing so -- simply because of the rapidly growing expenditures for research and because of the fact that, if companies are to continue to invest huge sums in research and assume the risks of supporting sizeable research programs, they must know whether their funds are being used effectively and whether the research output justifies the cost.<sup>20</sup>

More specifically, the company management wants to know whether the research investments are worthwhile at all, how the returns compare with those which could be realized from alternative investments, and whether the effort is maximally effective. The management desires to judge the past performance or output of research in a manner which will provide guidance for future research and operating effort.<sup>21</sup>

Going even further, the research director of one of the nation's largest companies contends that research itself is of little value if its results cannot be measured:<sup>22</sup>

In the professional work of the research scientist, measurement is an essential activity.  
— A theory of nature is not truly descriptive until

it becomes quantitative, nor is a scientific observation fully meaningful until numbers can be assigned with the phenomena under study. [Measurement of the functional work of scientists]... are of necessity if scientific research is to grow in magnitude and contribution to American industry.

The somewhat excessive number of quotations given in the preceding paragraphs represents but a few of many that could have been cited to illustrate the generally broad consensus held by most authorities on the research and development function: first, that the evaluation and measurement of research activities is complex and difficult; and second, that it is nevertheless necessary. Granted these two points, the question arises as to how the effectiveness of research performance can be measured. This brings in a variety of other factors which must be considered, such as the nature, the types, the levels, and the stages of research to be evaluated, interwoven with considerations of the most applicable methods of evaluation and the effects of evaluation upon the research personnel involved. Although the primary focus of this paper is on the evaluation of industrial research and development, these broader areas will first be addressed before returning to the main topic of interest.

### Nature of Research

Research is systematic investigation designed to seek answers to questions or solutions to problems. Although the word itself suggest a re-examination or a repeated search, the following provides a better understanding of its meaning:<sup>23</sup>

---

What, then, is research? It is not mere RE-search (although plenty of that has to be done); it is more than hashing over the work of others. It is re-SEARCH, fresh inquiry into and seeking for something new and perhaps better, despite the fact that "better" is a value-word, and therefore tabu in some academic mores. Here originality comes in.

Research is purposeful, objective, and systematic analysis, distinguishable from informal observation by its conceptual methodology. The research method is purposive, for it is the planned seeking of answers to specific questions or problems. It is objective, for it minimizes bias and prejudice in its processes. It is systematic, for it is carried out step-by-step to follow a well-defined plan. And it is analytical, for it reports conclusions based upon the principles of logic rather than a summary of unclassified facts.<sup>24</sup>

### Levels of Research

The levels of research may be broadly categorized as pure, basic, applied, and developmental research. Pure research is fundamental inquiry. It is largely an end in itself, gathering facts for the simple reason that facts are worth gathering, and unconcerned whether or not the facts are of immediate use in solving an existing problem. Pure research simply seeks to add to the accumulation of knowledge and understanding by providing answers to general inquiries. Basic research, on the other hand, investigates broad questions, and seeks to provide answers that may be applicable to many and varied practical problems. Although its findings are generalized, they may be practically

applied to various situations. Applied research differs from basic research only in its specificity of investigation. It is used to study a specific question or problem area (which may be either simple or complex) in order to provide solutions which have immediate and practical applicability. Developmental research is the last stage in the chain of incorporating more basic research findings into operational practicality. Its purpose is the resolution of any final problems standing in the way of actual application of the research findings into a usable product.<sup>25</sup>

There is no clear line of demarcation between the various levels of research, for one may lead to or blend in with another. What is defined as pure research may in actuality be basic, and what is designated as basic may really be applied research. Especially hazy is the boundary between applied and developmental research, for these two are often considered as synonymous. The problems of classification of research levels are illustrated in the following two statements:

We define basic research as the search for new phenomena and relationships in nature and applied research as the exploration for and creation of product possibilities out of science and technology. We are excluding development and engineering from our considerations because we assume that when a project reaches the development stage, the worth of the research has already been established. We must remember, however, that basic research, applied research, and development and engineering form a continuum. They are often carried on simultaneously in such a manner as to prevent simple classification of a project.<sup>26</sup>

The phase which occurs earliest in the development cycle is pure or basic research. Its



result is creation of new knowledge, which, in the physical sciences, is concerned with structural and dynamic properties of matter. It is the process of creating mathematical equations and/or experimental procedures which explain observable (man-made or nature-made) phenomena.

Applied research, which follows, is the application of knowledge to the solution of previously unsolved (usually generic) problems. It can either be device-oriented . . . or it can be technique-oriented. . . .

The fact that the degree of uncertainty (of the outcome of a proposed project) can vary over a wide spectrum gives rise to a major difficulty in defining clear-cut separation between applied research and activities which precede and follow it. At one extreme, it is not uncommon to find applied research proposals whose outcome is so uncertain as to require, in fact, creation of new knowledge and which should, therefore, be recognized as basic research. . . . Confusion as to the true nature of applied research occurs most frequently at the other end of the spectrum of uncertainty, where it may be confused with a later stage in the product development cycle, which is called product development.<sup>27</sup>

These two statements -- the first by G. K. Teal and the second by I. H. Ansoff, both recognized authorities on the subject of research -- clearly illustrate the problem involved in trying to classify the levels of research effort.

### Categorizing the Levels

The degree of uncertainty of the results of research appears to be the basic criterion for categorizing the level of research effort. At one end of the spectrum of uncertainty is fundamental research and at the other end is developmental research. Based on this spectrum, James

R. Bright has identified technical innovations as falling into one of the four following categories:<sup>28</sup>

Routine Improvements . . . are minor engineering changes in which the technical knowledge for design, implementation, and application are known and predictable. No research is required; nor is exploratory development needed.

Major Advances . . . are substantial changes in which the technological direction and needs are within the "state of the art," although not previously performed. . . . The necessary technology is identifiable and seems to be achievable. The technical program can be designed and planned with some certainty. Little new scientific knowledge is required, although engineering development will be needed to resolve the design details.

Technological Breakthrough . . . involves a radical change or improvement that requires new scientific knowledge and consequent development work. Applied and, possibly, basic research is necessary. The technological answers seem to be feasible, but are, for the moment, unconfirmed or unknown.

"Blue-sky" Projects . . . are technological proposals in which the means of accomplishment or the end results are largely unknown. Substantial new scientific knowledge and consequent engineering knowledge is required. Basic research and applied research is indicated. There is generally a compounding of many technological uncertainties in knowledge, equipment needs, and results. Therefore predictability is very low.

Yet another author has segregated industrial research activities into the following project categories:<sup>29</sup>

Pure research, i.e., direct research or experimentation on general problems having no particular connection with the various products currently being manufactured by the plant.

Projects directing experimental or developmental effort toward the creation of new processes or new products or groups of products currently being manufactured by the plant. . .

Projects directing experimental or developmental effort toward any improvement to a specific product already being manufactured by the plant or an improvement of an existing process. . .

All further work beyond the developmental stage necessary to get a new product, model, or item of equipment ready for normal production and sale.

Projects for the purpose of designing and constructing new types of equipment or improvements to existing equipment which shall be used in our manufacturing processes and which will effect a change in any existing process in the plant.

#### Interrelationships

Although the majority of present research expenditures are made in the areas of applied and developmental research, it is important to remember that all levels or categories of research are needed, since each contributes to the improvement of understanding and practice. Though more importance may be placed on one type of research over another in a given time or place, advancement in technology can easily be retarded if one type is emphasized to the detriment of another. All are interrelated and support one another: pure research often reveals new facets of understanding that have important implications to the solution of practical problems, and applied research frequently provides essential data which opens up new vistas for pure research inquiries. Thus all levels of research are needed and must be jointly conducted for progress to be achieved.<sup>30</sup>

### Implication for Evaluation

Assuming that the spectrum of uncertainty ranges from a great deal of uncertainty at the lower end of the spectrum to a fairly high degree of certainty at the upper end, then the principal implication for evaluation of research performance lies in the fact that research results may be measured with some degree of confidence at the upper end of the spectrum (i.e., in the areas of greater certainty) whereas at the lower end (i.e., in the areas of greater uncertainty) the measurement of research effectiveness becomes progressively more difficult.

Thus, developmental research, falling within the upper end of the spectrum of uncertainty, is closely concerned with producing a product whose output may be reasonably well compared to the inputs required to produce the product. For this level of research, more quantitative means of evaluation may be effectively employed. In the absence of a profit-motive (such as the research performed by or for governmental agencies), the item which is produced may be evaluated against its design specifications to determine the effectiveness of the research effort. When the developmental effort leads to a good produced for sale, the resultant profits may be further quantitatively measured against the development costs to determine research effectiveness. On the other hand, the fundamental levels of research, falling within the lower end of the spectrum of uncertainty, remain more difficult to measure, and qualitative or subjective yardsticks must be used to evaluate how well the research effort has succeeded.

### Types of Research

Many various types of research effort are conducted by many different agencies and organizations, but most research endeavors could be included under the following categories: research conducted by or for the government on defense needs and on health, welfare, environmental, and related programs; research conducted by business enterprises to maintain or advance their competitive position or prestige within their given industry; and research conducted by universities, non-profit institutions, and private research organizations for any of these purposes just cited or, simply, to increase and broaden basic knowledge.

#### Government

Approximately half of the total research expenditures within the nation are made by the government. A large proportion of these government expenditures are primarily for defense and space research, but an increasingly greater amount goes to other areas of research, such as atomic energy, food and drug, agriculture and forestry, natural resources, public health, and similar areas of domestic concern. Although much of the government-financed research is performed within government laboratories, a great deal is done by non-profit research institutions and by colleges and universities. In fact, one survey revealed that almost three quarters of the funding for research by academic institutions came from the Federal government.<sup>51</sup>

Although student protests within recent years against the involvement of educational institutions in government-

sponsored research programs have resulted in the curtailment of such research by some universities, it is still expected that most of the institutional research efforts will continue to be government-funded within the foreseeable future.

#### Academic Institutions

One study has revealed that colleges and universities are expending nearly \$1 billion per year on research efforts of various types. Most of the pure and basic research within the United States is conducted by academic institutions, with about half of their effort being devoted to the pursuit of fundamental knowledge.<sup>32</sup> Many institutions have established bureaus of research that conduct applied and operational research for individual firms within industry and for trade associations. A portion of their income from such projects is used to finance pure and basic research conducted by individual faculty members of various departments. In more recent years, extensive use has been made of research organizations established separately from the academic institutions, either as mere extensions of the institutions themselves, or as quasi-independent bodies or, even, as separate legal entities, receiving funds primarily by selling their services to the government, business enterprises, and foundations.<sup>33</sup>

#### Private Organizations

Private research organizations, ranging from non-profit research institutes to research consultant firms, expend approximately \$100 million per year on research efforts,



according to one estimate.<sup>34</sup> [Although often included in this category of research organizations are the various business and industry associations that conduct some research projects, they are more appropriately listed under industry research efforts.]

These private organizations, whether they be non-profit or commercial, depend upon clients for their business, and they sell their services to governmental agencies, educational institutions, and business firms. Their orientation may be toward fundamental scientific research; they may be oriented toward applied and operational research; or their concern may be with national and economic research. However, most of the endeavor of the commercial laboratories is of the applied and developmental research level, while that of the non-profit institutes lies at the other end of the research spectrum.<sup>35</sup>

### Industry

Research expenditures by industry account for nearly half of the total research expenditures within the nation. Although something like 95 per cent of this funding goes to applied research and development, the remaining amount expended on basic and fundamental research nearly equals the amount funded by academic institutions for the same purpose. Although much of this basic research within industry is performed in company research organizations, a significant amount of the industry research funding goes to research conducted by employer associations, private organizations, and academic institutions.<sup>36</sup>

### Implications for Evaluation

The principal implication, concerning the evaluation of research effectiveness, of the types of agencies which conduct research is essentially the same as that arising from the spectrum of uncertainty. The more basic the research performed by academic institutions, some government laboratories, and a few business enterprises, the more subjective or qualitative must be the means of measurement of these research efforts. On the other hand, the applied research and development efforts, falling within the more certain end of the spectrum of uncertainty, may be gauged by more quantitative methods of evaluation.

### Stages of Research

The stages of research are what has often been called the developmental cycle -- running from initial consideration of proposed research through in-process activities to the completion or termination of the project. Many tend to confuse the stages of research with the levels of research, thinking that the proposed research stage is akin to the fundamental research level, that the in-process stage is comparable to the basic or applied research level, and that the completion stage is similar to the developmental level. While fundamental research often leads to the development of products, this should not be confused with the life-cycle or stages of progress of research efforts in the developmental cycle. Any research project--be it fundamental or developmental--usually goes through

a cycle of activity stages, initially commencing with consideration of the proposed research and ultimately concluding in some sort of research results.

To determine how effective is the research effort of organizations, some means of evaluation must be employed to measure the research program at the various stages of their developmental life-cycles. As one authority has stated, "Evaluation of any activity generally implies that there is a predetermined set of values or philosophy that will serve as a criterion in making the evaluation. And evaluating a product research program is no exception."<sup>37</sup> Thus, techniques for research evaluation must relate to these predetermined criteria.

#### Implications for Evaluation

Techniques for pre-appraising proposed projects are primarily concerned with establishing a set of values and criteria to assist in the evaluation and selection of alternative research proposals and against which subsequent effort and results may be measured once a project is inaugurated. Techniques for evaluating in-process activities normally attempt to measure the current status or degree of completion of research efforts against criteria initially planned for the various points in the developmental cycle. And, techniques for post-appraising projects try to evaluate the effectiveness of research results viewed in the light of the pre-established goals and criteria. These techniques of evaluation range across the entire spectrum of methodology -- from the qualitative to the quantitative ends of the spectrum.

## Methods of Evaluation

Although there are many different methods of evaluating research potential, performance, and effectiveness, they may be grouped into three basic categories -- quantitative, qualitative, and some combination of the two.

### Quantitative Methods

The quantitative methods typically use mathematical equations or models coupled with dollar evaluation (when available) to arrive at some figure which describes the merit or effectiveness of research efforts; "...they range all the way from simple formulas -- which use company profits for a given period of time as the numerator and research costs as the denominator -- to more sophisticated approaches involving present worth and discounted cash flow."<sup>38</sup> They attempt to measure the contribution of the research program by means of mathematical equations relating to the profits from research created, improved, or cost reduced products, techniques, processes, materials, or, even, good-will, and to the savings engendered from costs avoided or income received from royalties.<sup>39</sup> These are compared with the costs of operating the research function, and these comparisons, in turn, are often compared to similar data on other research organizations.<sup>40</sup>

Proponents of the quantitative methods of analysis claim that sound, objective formulas numerically present the most accurate possible estimates of research costs and returns and that, in simplifying the details of research programs into easily understood dollar figures and ratios, they help gain the support of top management for research efforts.<sup>41</sup>

Opponents of quantitative measurement devices cite many difficulties in their use: It is often impossible to correlate costs with performance and one research organization with another, because of differences in accounting practices, size and functions performed, terminology used, or, simply, the absence of valid data. Quantitative methods can, at best, be used only to a limited degree for appraising research projects; often the data needed for the formulas are of such questionable accuracy, or unavailable, that the results are of little value. The time-consuming characteristics of some of the more sophisticated techniques often outweigh any advantages gained by their use, and the rigid application of inflexible formulas frequently gives misleading results which careful judgment would not have done. Since the elements of the formulas themselves must be determined by individual judgment initially, then it would seem that judgment alone is adequate without resorting to the use of complex and time-consuming equations.<sup>42</sup>

The opposing views toward the use of quantitative methods of evaluation are not totally irreconcilable. Within each group of statements there may be found some items of merit which, when considered in relation to the variable dimensions described by the levels of research effort and the stages in the developmental cycle, might be summarized as follows: The quantitative methods are best used in the applied research and developmental end of the spectrum of uncertainty. Their use for evaluating pure and basic research efforts is of questionable value, as is their use in measuring the performance of non-profit

motivated research endeavor. And, they are normally of greater value in the post-appraisal stages of research evaluation than in the pre-appraisal stages where greater uncertainty of data is prevalent.

### Qualitative Methods

Because of the weaknesses cited in the use of quantitative methods of evaluating research efforts, many organizations heavily rely on qualitative measurements or subjective judgment. Qualitative evaluation is usually the result of a composite judgment of qualified and responsible management and research personnel arrived at through logical but nonmathematical procedures and devices. Such procedures normally consist of a tiered or pyramidal layer of appraisals by all concerned with the research function. Judgments, based on personal experience and "feeling for the situation," arrived at through day-to-day technical management activities and periodic formal reviews, are used to evaluate the research effort. Often, certain semi-quantitative yardsticks, such as comparisons with the results obtained by competitors or by others within the organization or with the relationships of achievements to stated objectives, are employed to support qualitative appraisals.

The advantage of qualitative methods is that they rely on the judgment of experienced and responsible research personnel and thereby avoid some of the pitfalls of the more inflexible quantitative measures. However, if qualitative analysts completely ignore the use of



appropriate numerical data which could be useful in evaluating research effectiveness, they bypass important tools which could be used to sharpen their subjective analyses. In fact, by merely formulating subjective judgments, qualitativists weigh many of the factors which are included in quantitative analysis.<sup>43</sup>

In summary, the qualitative methods are best suited to evaluating the fundamental end of the research spectrum. They can be used to advantage in measuring the quality of research results, the degree of research efficiency, the results of long-range research, the intangible products of research such as publications, and the non-profit motivated type of research such as that conducted by government laboratories. They may also be of greater value than quantitative methods in the pre-appraisal stages of the research developmental cycle.

#### Combination Methods

Obviously, the quantitative versus the qualitative methods are at the opposite ends of the measurement spectrum. Variations of one or the other, or combinations of the two, are normally employed. The use of one may be tempered by the use of the other. The somewhat obvious advantages of the use of combination methods of analysis will be further discussed later in this paper.

### III. Evaluation of Industrial Research Endeavor

In reviewing the literature relating to the evaluation of industrial research efforts, it was noted that statements were often made that, "our company uses the XXXXX method for appraising its research programs," or "we've found the best technique to evaluate our R&D is XXXXX." Seldomly did these authors state where in the developmental cycle was their particular method of evaluation employed, nor for that matter at what level of research effort was the means of measurement used. In an attempt to create some degree of order in evaluating the different methods and techniques employed for evaluating of industrial research efforts, these various means have been classified as to their applicable use at various points in the developmental life-cycle; i.e., the methods have been classified as to their applicability for pre-appraisal, in-process measurement, and post-evaluation.

#### Pre-Appraisal

Widespread use of both quantitative and qualitative methods of evaluation are found for pre-appraising alternative research proposals.

Some of the more qualitative methods are concerned with such fundamentals as linking proposed research to company goals. For instance, the assistant vice-president of research and engineering for one of the nation's most progressive electronics firms has stated:<sup>44</sup>

The heart of the problem of selecting worthwhile research projects in an industrial laboratory is matching research to company goals. Company goals should take advantage of economic, social, and technological trends and be based on considerations such as management's aspirations and the company's maturity, economic condition, and capabilities in all aspects of creating, making, and selling products....

To be considered worthwhile, a project must have the following general characteristics:

1. It generates good industrial technology.
2. It produces new product ideas or ideas for product improvement.
3. It enhances company prestige in the scientific community.
4. It has a reasonable probability of success.
5. It challenges, extends, and trains technical personnel project leaders.
6. It has the enthusiastic support of a project manager.

Another company vice-president for research and technology established the following criteria for consideration in evaluating research proposals:<sup>45</sup>

Criteria: Will the results of the project, assuming success in the research phases, produce--

1. A saving or increase in profits commensurate with the anticipated cost of research and development?
2. An improvement in quality essential to the maintenance of the company's competitive position?
3. An increase in the volume of business on currently produced items commensurate with the cost of the research and development?
4. A substantial improvement in consumer, customer, or government relations?
5. A new product?

If a new product: Will it be--

1. One that fits logically into the company's existing lines of business?
2. One for which there is an anticipated market demand?
3. One for which raw materials are available?
4. One which can be manufactured and sold profitably over at least a normal payout period?
5. One that can be produced with existing plant facilities; or, where new facilities are required, one where the capital investment and resulting depreciation charges are not excessive in relation to the volume of business and selling price than can be anticipated?
6. One for which the company possesses the manpower and facilities to develop, manufacture, and introduce?
7. One for which the development and introduction costs can be amortized in at least a normal payout period?
8. One upon which the return on investment can be estimated with reasonable accuracy?
9. One which the company is financially in a position to develop and introduce?

Along similar lines in regard to the consideration of the development of a new product, another author lists the following questions to be answered:<sup>46</sup>

1. What reasons are there for moving into the new type of business?
2. What is the relative status of present products?
3. Which new markets are most closely related to those now being reached?
4. What is their expected rate of growth?
5. What is the status of competition in the new business?
6. Are there needs that are not being met by available products?
7. What are the capital requirements for getting established?

8. What are the requirements for success in the new business?

9. Are we properly staffed in kind and number to manage a successful new business?

10. Should expansion take place through acquisition or by developing our own products?

One research consultant suggests the use of a checklist approach by which each proposal should be subjected to rigid tests to determine technical feasibility from an engineering and manufacturing viewpoint, its marketability based upon customer tastes and distributive requirements, and its profit potential based on projected sales volume, price, cost of production, and cost and availability of capital. In his checklist he includes many different factors and lists the probable source of information on each of these factors. The major headings in the list of factors include: research proper, production, customer's specifications, potential demand, scale of plant, location of plant, operations, competition, pricing to achieve objectives, capital requirements and profits, relation to long-term plan, effect of change on process research, measuring gains, financing capital requirements, and risk. The major sources of information for these factors include: research, engineering, market research, economic research, manufacturing and sales, new product development, traffic, accounting, budgeting, and treasurer.<sup>47</sup>

James R. Bright goes into considerable detail in his relatively recent book on technological innovation in discussing the appraisal of the potential significance and possible risks in technological innovation. He presents detailed listings of various considerations in

each of these areas. One listing, relating to conditions for achieving economic value from potential technological advances, includes under areas of consideration the effects on product performance, effect on the inputs required to produce or support the innovation, effect on users, and effect on economic society. Another listing relating to risks in innovation includes checklists of risks in technical areas, including scientific, engineering, and production risks, risks in marketing areas, interference risks, timing risks, and obsolescence risks.<sup>48</sup>

In general, then, the criteria for pre-appraisal of contemplated research programs include a measure of economic potential, taking into account sales and profit potential, return on investment, and intensity of competition; along with a measure of marketability or customer acceptance and a measure of technical feasibility, taking into account technical obstacles, cost of research, time span required for research, and the possibility of obsolescence brought about by the introduction of a competitive product.<sup>49</sup>

Despite the previously discussed limitations of the use of quantitative methods in the pre-appraisal of research projects, many companies use various formulas to either determine or to assist in determining the potential value of the proposed program. One of the better known equations is the "Index of Return Formula" devised by Fred Olsen of Olin Industries.<sup>50</sup>

$$IR = \frac{EVR \times ECS}{ECR}$$



where:

- IR = Index of Return; which determines whether a project is worthwhile or should be abandoned.
- EVR = Estimated Value of Research, if it succeeds; the summation of the full value of savings for one year for a process improvement plus 2 per cent of the sales value for two years of a product improvement plus 3 per cent of the sales value for five years for a new product.
- ECS = Estimated Chance (or probability) of Success.
- ECR = Estimated Cost of Research.

Experience has shown that the index of return should exceed the value of 3 before the project is considered worthwhile and the project should be abandoned should the index fall below 3. The percentages and time periods for the EVR term are arbitrarily assigned and have been changed in the years since the formula was first applied.

A variation of Olin's Index is the Miller Variation suggested by Theodore Miller, which uses as a numerator the product of the following:<sup>51</sup>

(Percentage chance of technical success) X  
 (Percentage chance of commercial success) X  
 (Estimated unit sales per year) X  
 (Profit per unit) X  
 (The square root of the life of the product  
 stated in years)

The formula yields a "product number" which is used as a basis for comparing various research proposals.

A formula described by Ralph Manley of General Mills is used as a guide to the amount of money that a company can afford to spend on research leading to a new development:<sup>52</sup>

$$R = 2 (0.01 YNS - P - W)$$

where:

- R = research and development costs before taxes,
- P = plant investment,
- W = working capital required,
- Y = recoupment period (period in which costs must be recovered),
- S = annual sales volume of new product, and
- N = minimum acceptable net profit on sales.

Using representative figures provided by General Mills, a \$1 million plant (P) produces \$1 million worth of goods per year (S); to run such a plant, \$0.5 million working capital (W) is required; the payout period (Y) is 5 years; the minimum return on sales (N) is 6 per cent; 50 per cent of research and development costs are tax-deductible (the factor 2 in the equation). The computed amount that can be spent on research (R) is \$600,000.

G. K. Teal, Assistant Vice-President of Research and Engineering for Texas Instruments, described an Index of Research Effectiveness used to quantitatively evaluate research projects. The index is the product of four other indices:<sup>53</sup>

$$\text{Index of Return on R \& D} = N / 25 S$$

where:

- N = Net profit during the life of the resulting product.
- S = R & D costs.
- 25 = Taken from statistics on return on R&D collected by the National Science Foundation.

$$\text{Index of Return on Assets} = N / 13.5\% A$$

where:

- N = Net profit during the life of the resulting product.
- A = Assets which were required.
- 13.5% = Based upon a judgment that a worthwhile product should make possible a favorable return of 13.5%.

$$\text{Index of Dollar Volume} = b / (B/25)$$

where:

- b = Billings made possible by the product.
- B = Total billings during the product's lifetime.
- 25 - Based upon a judgment that a worthwhile product should make possible a 4% increase in total annual billings.

$$\text{Index of Market Capture} = b / (M/2)$$

where:

- b = Billings made possible by the product.
- M = Total available market.
- 2 - Based upon the judgment that a worthwhile product should capture 1/2 of the total market.

If, when multiplying these four indices to obtain an index of research effectiveness, a factor of one or more is obtained, the project is considered a worthwhile investment.

The Calculated Risk Basis Formula is used to compute a project's rate of return to be measured against a standard which states an acceptable return:<sup>54</sup>

$$R = \frac{\sum_{i=0}^n [P(I_i)]}{\sum_{i=0}^n (O_i)}$$

where:

- $I_i$  = Net estimated income from the project's results in any  $i$ th year,
- n = The last year in which incomes are expected,
- P = Probability of receiving the  $i$ th income.

- $O_i$  = The net incremental investment outlay in any  $i$ th year,  
 $i$  = The year hence in which the income or outlay will occur, and  
 $R$  = The project's rate of return on total funds invested.

The Unadjusted Return Technique makes broad calculations of a project's potential return versus its costs. Projects are more desirable as  $R$  increases, the minimum  $R$  required to accept any project being 30 per cent before taxes:<sup>55</sup>

$$R = \frac{\sum_{i=0}^n (I_i)}{\sum_{i=0}^n (O_i)}$$

where:

- $R$  = The rate of return on investments in the project,  
 $I_i$  = The income the project will yield in each  $i$ th year,  
 $O_i$  = The incremental outlay required in each  $i$ th year,  
 $i$  = The number of years hence each income or outlay is realized, and  
 $n$  = The number of years hence the last income or outlay is realized.

The Hoskold Transformation computes the present worth of the incomes the project will yield for comparison with the present value of the project's actual and projected costs to determine if it is financially acceptable:<sup>56</sup>

$$P = \frac{D}{R + \frac{R'}{(1 + R')^n}}$$

where:

- P = The present worth of the incomes the project will yield if successful,
- D = The average annual incremental income yielded if the project is successful,
- R' = The average net return on capital invested in the enterprise,
- R = The current rate of interest on investments, and
- n = The number of years within which research costs must be recovered.

Present Value Techniques are often used since the  $(1 + R)^{-1}$  factor, shown below, is readily available in standard financial tables and projects can be ranked in order of ascending P to determine their relative desirability.<sup>57</sup>

$$P = \sum_{i=0}^n [C_i (1 + R)^{-1}]$$

where:

- P = The project's present value,
- $C_i$  = The net cash inflow in each  $i$ th year (net cash outflow in a year would make  $C_i$  negative),
- i = The number of years hence the cash flow occurs,
- n = The number of years hence the last significant cash flow occurs, and
- R = A discount rate which measures the rate at which the company would invest its money to obtain cash flows of equal risk in the same year.

A framework for evaluation of applied research has been proposed by I. H. Ansoff, which establishes two formulas based on criteria related to potential earnings and criteria related to investment. By assigning arbitrary scales to the individual criteria, both figures of

profit merit and of risk may be computed for each project and then the competing projects may be compared.<sup>58</sup>

$$FM_p = \frac{(M_t + M_b) \times E \times P_s \times P_p}{C_d \times J} \times S$$

$$FM_r = \frac{C_{1r}}{FM_p}$$

where:

Criteria Related to Potential Earnings

- I. Profit potential
  - Estimate of total sales (earnings) over lifetime (E)
  - Technological merit ( $M_t$ )
  - Business merit ( $M_b$ )
- II. Probability of success of project ( $P_s$ )
- III. Probability of successful market penetration ( $P_p$ )
- IV. Strategic fit of proposed project with other projects, products, and markets of the company (S)

Criteria Related to Investment

- I. Direct investment ( $C_{ar} + C_{pr} + C_{pd} + C_{pe} + WC + F = C_d$ )
  - $C_{ar}$  = Total cost of applied research effort
  - $C_{pr}$  = Total cost of product research effort (exclusive of applied research)
  - $C_{pd}$  = Total cost of product development effort
  - $C_{pe}$  = Total cost of product engineering effort
  - WC = Working capital required
  - F = Total cost of extra facilities required, such as staff, buildings, etc.
- II. Joint cost effect (J)
  - J = Savings factor in direct investment resulting from use or sharing of existing facilities and capabilities.



Despite the many quantitative formulas used in evaluating research projects, most companies seem to use them as aids in making more subjective judgments. Hugh F. Colvin, Executive Vice-President of Consolidated Engineering Corporation, pointed out that an important consideration is the standpoint of time. Although technical, manpower, facilities, capital, and estimated rates of expenditure considerations are important, the project that may be sound at one point in time may be quite inappropriate, for various reasons, at an earlier or later date.<sup>59</sup> And, Thomas Moranian, in his book, The R&D Engineer as Manager, emphasized that although a company may be in business to make a profit, return on investment is not always the governing factor in the selection process. At times a potential new product that does not have a comparable return on investment, but fulfills the need to broaden a company's line of products, may be selected because it helps to maintain and promote the general sales level; or a project improving the quality of an existing product may be selected because it enhances a company's position or prestige.<sup>60</sup>

#### In-Process Measurement

The measurement of effectiveness of research efforts on projects or products in the in-process stage of the developmental cycle appears to be much more difficult than either pre-appraisals or post-evaluations of research endeavor. Most means of measurement are actually instruments of control. Quantitative devices are essentially of the scheduling or budgeting type, and qualitative devices include all sorts of program reviews and progress reports.

Although quantitative scheduling devices can rarely be utilized to any great degree of success with the more fundamental or exploratory forms of research, much of the applied research and developmental effort may be controlled and measured by the use of Gantt charts, project milestone schedules, life-cycle analyses, or computer-reporting techniques.

Gantt charts are often used to control and measure the progress of smaller-sized research projects or are used in conjunction with computer-network systems in larger projects for more detailed analysis of actual versus scheduled performance within smaller segments of the total network.<sup>61</sup> Project milestone schedules are similar to the Gantt charts in that they show scheduled dates for completion of certain activities along with the percentage of completion to date.

The life-cycle method of control and measurement may be instituted for larger projects when sufficient data have been accumulated to determine the pattern of the initial cycle of the project development. This method gives a mathematical model of the overall manpower pattern of a project from the planning and specification cycle through the design, prototype, and release cycles of the life of the project. It is based upon an extensive study of R&D programs which found that the ratios of total manpower between two successive cycles are very stable for a particular type of operation. Once the pattern of the first cycle has been determined through means of computerized accounting procedures, the pattern for all the subsequent cycles may be determined by the following mathematical equations:<sup>62</sup>

$$y = k (1 - e^{-at^2})$$

$$y' = 2k a t e^{-at^2}$$

where:

- y = Manpower to date, in man-months.
- y' = Manpower for each month.
- k = Total effort for the cycle in man-months.
- a = Parameter governing the shape of the curve.
- t = Time from start of the cycle (in months).
- e = A constant (base of natural logarithms = 2.78128...).

Whenever the size or complexity of a program justifies their use, computerized scheduling and reporting techniques such as PERT, CPM, or RAMPS may be used for analyzing in-process research and development activities.

The PERT (Program Evaluation and Review Technique) system is used when a project requires a network involving hundreds to thousands of events, and it is also sometimes used for planning smaller projects having only a very few events. Although the PERT system may be used without employing electronic computers, its effectiveness is facilitated by their use. The PERT/TIME system is based upon the preparation of a network of specific milestone events connected by necessary activities leading from one event to another. The timing of the activities is normally based on a weighted value of three time estimates--the optimistic, the most likely, and the pessimistic--made by the scientist or engineer responsible for the activity. The longest path in the network going from one particular event to another is called the critical path, while the other alternate paths are referred to as the slack paths.

Should the estimated completion date resulting from the time span along the critical path be later than the required target date, resources may be transferred from the slack paths to the critical path or the network may be redesigned to eliminate certain tasks or to parallel various activities in order to correct the situation.<sup>63</sup>

The PERT/COST system translates each activity in the PERT/TIME network into manpower hours and related costs. The total network time can be translated into dollar figures for each activity and a budget may be developed for the entire project. The PERT/COST system permits the various alternatives to be considered in light of the costs involved, as well as the schedule considerations.<sup>64</sup>

Like the PERT/COST system, the CPM (Critical Path Method) takes costs into account. This method includes time and cost rates for concluding a project--a normal rate and a crash rate--which permit comparison of cost against varying durations of the project in order to select the optimum alternatives.<sup>65</sup> The availability of resources is taken into account in the RAMPS (Resources Allocation and Multi-Project Scheduling) system. This facilitates decisions regarding the allocation of resources whenever there are alternatives which may minimize the cost of completing the scheduled program.<sup>66</sup>

These quantified control systems are often criticized by various authorities who contend that too great a reliance is placed upon their use. Although the systems present mathematical answers which would seem to clearly delineate the available alternatives and direct

the selection of the optimum solution, they are, in fact, based on qualitative inputs and are thus no better than the quality of the inputs themselves. For instance, with the PERT system subjective judgements must first be made in establishing the network of events and activities and in making the time estimates within each path. Critics of these systems feel that a great amount of time, effort, and cost is expended in quantifying data which are based on little more than qualified judgment. The systems are only as good as their inputs and no matter how much they are mathematically manipulated or computer programmed, if the initial judgments establishing the data for the system were poor, then there is nothing but "garbage-in; garbage-out." Thus, many feel these complex quantitative systems are of questionable value in controlling and measuring research and development activities. They feel that the uncertainties involved in research work where there is a diversity of operations and the lack of any identifiable product unit indicate that no concrete measure of R&D output can be devised.

For these reasons many believe that other control devices must be used to evaluate research effectiveness. "What can be measured at present with a reasonable degree of accuracy is the amount of resources devoted to research and development. . . . Since the output . . . cannot be measured at present, the only way the volume of research activity can be described is in terms of its inputs."<sup>67</sup> Some believe the only quantitative system that can be used is that of establishing extensive budgetary and accounting systems which monitor the research expenditures, compare them to pre-established budgets, and report

variances between actual and budgeted rates.<sup>68</sup> In the Handbook of Industrial Research Management, Walter B. McFarland stated:<sup>69</sup>

Through the budget, management can control the total amount of money spent for research and it can direct expenditures to make sure that funds are spent in the way management wants them to be spent. This is accomplished by planning how available funds are to be used and by comparing current expenses with budgets to aid management in charge of research to keep actual expenditures within limits.

McFarland, Manager of Research for the National Association of Accountants, further stated that the purposes of cost control in its application to research activities are:<sup>70</sup>

1. To make sure that the plan expressed in the budget is followed by directing funds into projects of types desired.
2. To avoid spending research funds on nonproductive or nonresearch activities.
3. To stimulate an attitude of dollar-consciousness so that research personnel will attempt to perform as much research as possible for the funds available.
4. To keep the total spent for research within the limit set by the appropriation for the period.

Toward this end, research costs are classified and accounts are set up so that direct reports may be prepared from the established accounts. Such classification is designed to answer the following questions:<sup>71</sup>

1. How much was spent for research and development?
2. Who spent it? (i.e., classification by responsibility for cost control).
3. For what were the costs incurred? (i.e., classification by nature of response).
4. How was the effort of the research and development organization applied? (i.e., classification by project and division).

Once the budget has been established and accounts and classifications of costs assigned, data-processing operations may be inaugurated in order to prepare timely reports of variances between actual and budgeted expenditures to guide management decisions. For instance, cost control techniques may be used in conjunction with scheduling and milestone devices to facilitate the selection of possible alternatives. If the scheduling system indicates a serious delay that must be resolved by alternative means, the cost control data can help provide an evaluation of the expected cost of avoiding or reducing the impact of the delay compared to the cost of the delay itself. Raymond Villers cites two formulas that may be used for time and money variance analysis in this situation:<sup>72</sup>

After the optimum alternative has been selected two formulas can be used to summarize the situation and to show clearly how the technical progress is related to actual expenditures of time and money.

The formula for the time factor is:

$$V_t = O_t - (A_t - E_t)$$

where:

$V_t$  = the predicted time variance  
 $O_t$  = scheduled time for the project  
 $A_t$  = time already spent on the project  
 $E_t$  = amount of time that, on the basis of the revised schedule, will be needed to complete the project.

By adding  $V_t$  to the expected completion date, one can show the revised estimated completion date. This revised estimate must be compared to the company's commitments and to the needs of the market before it can be definitely considered as acceptable.



The second formula translates the first one into dollars.

$$V_b = O_b - (A_b - E_b)$$

where:

- $V_b$  = the predicted variance from the budget
- $O_b$  = the original budget for the period
- $A_b$  = the actual expenditures since the start of the project
- $E_b$  = the estimated cost of  $E_t$

The variance  $V_b$  may be due to the fact that  $A_t$  exceeds the original schedule. It may also be due to the fact that the research people have reported that  $E_t$  has to be revised. In any case, the variance  $V_b$  provides the information needed and will be included in the reports issued at the various levels of management.

While the budgetary systems may be used to help management evaluate possible alternatives, most authorities are quick to point out the limitations of such devices in the measurement and control of research and development activities. James B. Quinn has stated:<sup>73</sup>

The research budget is more useful as a planning device than for control purposes. Except in the most applied phases of research, estimating simply cannot be performed with sufficient accuracy to make the research budget . . . a suitable standard against which to determine research efficiency or effectiveness. . . .

In R&D budgeting, it is essential to recognize certain attributes of forecasts:

1. Effort on a project can be predicted, but results cannot be.
2. The research budget is likely to be accurate in the aggregate, but not in detail.
3. Research program budgets are accurate in the short run only.
4. Forecasts must allow for program flexibility.

In a similar vein, Walter B. McFarland cautioned  
that:<sup>74</sup>

Recognition should be given to the fact that the budget is . . . not a device to measure the output of research operations . . . While the application of effort can be controlled through the control of expenditures, measurement of results obtained in relation to specific expenditures is usually uncertain and often impossible. Certain goals in the form of new knowledge may underlie the allocation of resources when the budget is prepared, but expenditures . . . are not contingent upon realization of these goals.

Thus, like the highly quantitative control devices such as PERT, CPM, RAMPS, and others, the budgetary, accounting, and cost control devices are not a panacea in themselves. They are merely reporting techniques which provide management with quantitative data upon which to base qualitative judgments. For this reason many companies heavily rely on a system of reports which present both quantitative and qualitative analyses of the status of in-process research. The importance of these various status reports is emphasized in the following statement by Roy H. Walters, Vice-President of Research and Development for General Foods Corporation:<sup>75</sup>

These, then, are the two extremes: (1) the report we write before we start work to be sure we know what we are going to do; and (2) the final report in which we put down what we think we did. The one is too early to be of major use in keeping research on the track, the other is much too late. It is the great space between these two, the so-called during-the-fact report, that is most vital to research management.

Toward this end, Harley H. Bixler, Manager of Manufacturing Process Development at General Electric, stated:<sup>76</sup>

There are several ways to evaluate and report project status:

1. The progress-against-schedule report,
2. A periodic review of manufacturing which is attended by laboratory executives as well as by the project engineer,
3. A progress report which is reviewed by the project engineer and laboratory executives,
4. A continuing review by lab executives through visits to the project section.

In general, then, despite the limitations of any particular method of measuring research effectiveness, by employing a system of financial, technical, and scheduling reports and reviews, management can keep abreast of the progress or status of research endeavor and have available the necessary data for qualified judgment and informed decision-making in measuring, controlling, and directing the research and development function.

#### Post-Evaluation

The post-evaluation of research efforts is perhaps somewhat more concrete an evaluation than can be performed at the pre-program and in-process stages of the developmental cycle. In theory, the research has led to some end product -- a new or improved product or process, or, simply, research papers which provide some insight or new knowledge -- which may be evaluated against the actual expenditures incurred in conducting the research versus the original estimate of expenditures that was made when the proposed research was initially considered. Raymond Villers has described such evaluation as follows:<sup>77</sup>

In making a postappraisal, the several estimates of return on investment which were calculated in the first analysis of the project (and in subsequent analyses when progress was reviewed and additional R&D funds were requested) are compared with the several returns on investment which were achieved by using several figures for the investment in research. One such figure will be the total cost of research from the time the project was product oriented while other investment figures will include the cost of research since the various appraisals were made. The return on investment figure which is based on the total cost of research since the project was product oriented, even though some of the R&D was nonprogrammatic, is useful for management when it is combined with other postappraisals to determine the overall return from product-oriented research and to measure the benefits resulting from R&D activities.

In actuality, however, such quantitative post-evaluations of individual projects, described by Villers, are beset with difficulties. Although quantitative measurements of effectiveness may be used with some degree of success in evaluating the applied and developmental types of research, they are of little value in analyzing those projects falling in the more fundamental research end of the spectrum of uncertainty. In discussing the limitations of quantitative analysis methods, James B. Quinn stated:<sup>78</sup>

Although some quantitative devices purport to assess past fundamental-research programs, no company has found a satisfactory way to measure the impact of research performed less than three to five years previously.

Even in other areas, formulas provide answers based on figures that are themselves inaccurate. One can seldom predict precisely how much a new product will earn. No sound method exists for

apportioning credit for a product's profit between a company's research department and its other functional groups. And finally, one cannot allocate research costs among various products, except subjectively and imprecisely. However impressively mathematical they look, formulas based on such figures provide answers no sounder and no more trustworthy than the personal judgments that underlie their figures.

Walter B. McFarland, in discussing research cost accounting and control, listed several problems in matching research costs with sales income, among them the following:<sup>79</sup>

Benefits from specific research expenditures are always uncertain and years may pass before success or failure of a project becomes apparent. . . . Many projects which fail to accomplish their objectives yield incidental knowledge which proves to have great value at some future time. Moreover, the useful life of knowledge gained by research cannot be predicted with sufficient reliability to guide amortization of costs over the future periods benefitted.

For these reasons, many organizations make little attempt to quantitatively evaluate individual research projects, but try instead to measure the output of the total research function. Their approach is to view research success in light of overall company progress, without arbitrary establishment of payout and amortization periods and allocation of research and overhead costs for a particular project and without artificial apportionment to the project of the benefits and profits resulting from the ultimate production and marketing of the research results. Many different formulas and quantitative measuring devices are used by various companies

and organizations to evaluate the overall effectiveness of their research efforts. Among them are the following: 30

Comparison of research expenditures with sales gain over a period.

Comparison of research expenditures with the ratio of sales of new products to the sales of all products.

Comparison of research expenditures with the change in market share over a given time period.

Comparison of research expenditures with selling, advertising, or manufacturing expenditures.

Comparison of the rate of net profit from investment in research with the current rates of return on plant, property, equipment, or invested capital.

Comparison of the number of patents with those of previous periods of time or with those of competitors.

Number of disclosures to the patent attorney by the research personnel.

Number of scientific papers presented outside the organization.

Amount of royalties received from other companies from licensing of research-developed products.

Dollars of sales from new products.

Trend of new product sales measured as percentage of total annual sales.

Increase of the company's share of the market for specific new products.

Ranking of the company within its industry and/or market.

Comparison of the company's research position with that of other companies by various research intensity ratios, such as the number of people engaged in research or by dollars of R&D expenditures against operations in terms of employment or in dollars.

In a survey of 37 laboratories conducted by the Massachusetts Institute of Technology, A. H. Rubenstein

reported the following categories of criteria used to judge research and development work:<sup>81</sup>

		<u>Number of Companies</u>
(1)	<u>Related to effect on sales volume or revenue</u> ..... Increased business; increased output without increasing investment; share of the market; percent of products from research; consumer acceptance; effect of new products on old product sales; new customers.	19
(2)	<u>Related to effect on savings in materials, labor, or other costs</u> ..... Royalty payments saved; use of by-products, wastes, idle facilities or personnel, or less profitably employed facilities; reduction of product line; closer control of manufacturing quality; better process yields, etc.	17
(3)	<u>Related to effect on profits</u> ..... Profit on research versus non-research products; profit and loss analysis for whole R&D effort; payoff time on projects; percent return on investment.	13
(4)	<u>Related to time and cost of the technical solution</u> ..... Frequent re-estimates of time and cost; progress on project or program phases; actual versus budgeted expenses; actual versus scheduled progress; proportion of budget spent versus progress.	28
(5)	<u>Related to customer satisfaction</u> ..... Number and nature of complaints; broadening of product line.	10
(6)	<u>Related to information output</u> ..... Number of valuable ideas; percent of ideas from inside laboratory; learning about new processes and materials;	17



sources of new ideas; training individuals; development of specifications; evaluation of information output to application groups; information developed for sales; repeat requests for work.

- (7) Related to success of technical operations ..... 16  
Number of problems successfully handled; number of patents--written up, applied for, granted; number and nature of project failures.

Less quantitative evaluations are employed by many organizations. The American Institute of Management uses no formulas in analyzing research success; instead, it attempts to determine those aspects of company progress which may be properly credited to research by asking the following questions:<sup>82</sup>

1. What technological innovations has the company made that are now used in its industry? What basic discoveries?
2. What equipment or methods has it developed for its own operations as a result of research?
3. What patents have been granted? How many each year?
4. What proportion of present revenues derive from products developed by research? What proportion of the company's products derive from its research?
5. What change in market position can be attributed to research?

A broader qualitative approach is that taken by E. Duer Reeves, Executive Vice-President of Esso Research and Engineering Company, in the following two statements:<sup>83</sup>

. . . modern industrial research is only a part of an integrated company effort. Trying to measure its effectiveness by how much money the

company makes or by how many new products are put on the market is like giving a football quarterback all the credit every time his team scores a touchdown.

There are five questions I think pretty well define the job of the research organization and I think the answers to them give a pretty good idea of just how effective it is:

1. Does the research organization know what technology its company needs?
2. Is the research organization creating the needed technology?
3. Is the research organization helping to get research results applied?
4. Are the research organization's operations efficient?
5. Is the research management an effective part of the executive team?

Perhaps the broadest qualitative approach toward measuring the effectiveness of the research organization is presented by Warren C. Lothrop, in his book, Management Uses of Research and Development:<sup>84</sup>

I believe that the R&D organization must be judged in its own environment. This can be considered on three levels: the national or cultural environment, the industry environment, and the company environment. . . . We should not judge the worth of these three environments, or societies, in considering the function of research and development; rather we should value the function in terms of concrete accomplishment that serves the needs of that particular society. By "society" I mean any group that shares a common purpose. Such groups can be national groups, industry groups, or company groups. If the technical organization is a part of one of these groups, its purpose must be in accord with the purpose of the group, and its accomplishments must be judged in terms of its benefits for those who compose the group. In other words, it must advance the needs of its own society; otherwise it has no validity or real purpose. Evaluation of its success will naturally be in terms of how satisfactorily it meets these social needs.

### Individual Creativity

Whereas the methods of evaluation discussed previously are means of measurement relating more directly to the research organization's objectives and results, many measurement systems are related to the evaluation of the ideas and performance of individuals within the research organization.<sup>85</sup> Merit, contribution, and productivity rating systems are often used to measure individual research efforts.<sup>86</sup> Other methods, including quantitative measurement of research proposals submitted, papers published, patents applied for, and similar measures of individual creativity are also used.

However, the value of these particular evaluation methods is being increasingly questioned by behavioral and organizational scientists, and many think that their use may even be detrimental to the effectiveness of the research effort. For instance, in his book, Organization and Innovation, Chris Argyris asks, ". . . what is the meaning of criteria used for assessing creativity that depend upon superiors' ratings, papers published, and projects finished?"<sup>87</sup> Few researchers believe the ratings by either supervisors or peers have any validity, either because of the pressures on the rater himself or his lack of knowledge and understanding of the work the researcher is performing. Argyris describes a self-maintaining system leading to greater and greater ineffectiveness: The imposition of research evaluation systems engenders psychological reactions of defense and distrust which lower interpersonal competence and research effectiveness, which causes management to impose still more controls

which create deeper defense reactions which lower research effectiveness, and so on, infinitum.<sup>88</sup>

Upon reviewing recent trends of increasing pressure upon research organizations to become more efficient, Argyris feels that:<sup>89</sup>

Increasing intervention by the line departments, tighter research budgets, more systematic evaluating research, more rigorous (hopefully) quantitative indices for evaluating research payoff, more use of charts to control the flow of research, closer links with the marketplace and a weaker tie with basic research, and finally an increase in the use of top-level committees to oversee research, can be expected.

I question if these activities will solve the problems of research organizations. . . .

As budgets tighten, pressures increase, assignments to work become (from the researcher's point of view) more random, evaluative procedures increase, the researcher is forced either to give up and permit himself to be pushed and pulled as the pressures go, or to retreat into his own work and insulate himself from others. . . .

It seems to me that the strategies of tighter controls may increase the control that management has over already (and increasingly) inefficient activities. The unintended consequences . . . will develop and strengthen the undesirable and uncontrollable defensive activities that can make the organization less flexible, changeable, open, and innovative. It is not too farfetched to predict that if these processes continue long enough, they could help to make research organizations sick organizations.

Argyris believes that the imposition of necessary controls is not the problem; rather it is one that is concerned with the climate of the organization itself:<sup>90</sup>

I am not implying that controls, clear-cut objectives, and valid evaluation procedures are not useful. They are necessary, and the less defensive an organization is, the more it can generate and accept its own controls. I am implying that few committees will operate effectively, few evaluation procedures will tend to be accurate, few research schedules will tend to be realistic, and even fewer researcher-customer relationships will be made effective by these corrective measures. These measures do not get at the heart of the problem. At the core of the problem is the basic tendency for researchers to be uncomfortable with feelings, openness, and risk taking in interpersonal relationships. This leads to a research culture that will tend to reward conformity, mistrust, antagonism, and fear of risk taking. As fear, mistrust, and frustration with others increases, the individuals begin to question the motive of others.

As an answer to this problem, Argyris urges that organizations must examine themselves and cast off their organizational defenses:<sup>91</sup>

What is needed is that the researchers, especially the research administrators, be helped to increase their interpersonal competence and decrease the incongruence between their values and their behavior. . . . Each organization . . . must look within, at precisely those factors that it has been unable, to date, to discuss openly. An organization can go from one that is problem-creating to problem-solving, if its members are able to own up to how they really feel, become more open and receptive, take more risks and support the norms of individuality, concern, and trust.

These views held by Argyris were extracted in rather great detail because they pose important questions about the measurement and control of research efforts. The focus of this paper has thus far been upon the methods

and techniques that may be employed to appraise, measure, and evaluate the research performed within an organization. The implicit assumption has been that research effectiveness can be measured by some sort of method or device. Argyris raises the question that the use of any method or technique of measurement and control may be counterproductive if the organizational climate is not conducive to the use of such devices. Thus, merely by trying to measure research effectiveness, the effectiveness itself may be reduced as a consequence.

Those who are concerned with the evaluation of research and development activities might take comfort in thinking that Chris Argyris is alone in his views. Unfortunately, however, many others share his opinions and hold similar views concerning the use of controls (and associated measurement devices) in scientific, research, and highly technically-oriented organizations. For instance, Louis B. Barnes, Division of Research, Harvard Graduate School of Business Administration, questions the usefulness of management orientations which stress profits, productivity, and practicality to the exclusion of other values. In light of the negative consequences of such actions in technical groups, Barnes arrives at the following conclusions:<sup>92</sup>

1. Conditions of high status congruence in technical groups may offset a desired output of ideas, improvements, and product development.
2. Management may defeat its own goals by stressing organizational values to groups whose backgrounds encourage their seeking other, more distant reference groups.
3. Reward systems in relatively closed systems may artificially reward an individual for his social status rather than for his actual work contributions.

4. Organizational structures that formally restrict an individual to a relatively narrow subgroup activity may generate subgroup competition and block individual development.

5. "Span of control" concepts in technical groups may introduce supervisory control limits at the expense of subordinate autonomy and development.

6. Highly structured formal policies and procedures run the risk of confusing ends with efficient informality.

7. Competitive conditions within technical departments may be avoided by structures and procedures which help establish departmental goals rather than subgroup goals.

8. Formal follow-up procedures that stress productivity may do so at the expense of a superior's capacity to help subordinates achieve that productivity.

9. Relatively closed organizational systems tend to encourage influences, controls, and interactions that separate management logics from subordinate needs and contributions.

10. Management probably can hold a supervisor accountable for his group's productivity and satisfaction only if it explicitly takes account of the relationship between the supervisor and his own superior.

The consensus of many is that the standard control and evaluation procedures applicable to most large scale organizations are inappropriate for research efforts. An environment comparatively free of structural rigidities is more conducive to scientific creativity.<sup>93</sup>

In studying barriers to innovation, Sayles and Webber found that the innovator is often constrained by organizational structure, tradition, and inertia, and that creativity in the industrial environment is as much a function of structural design, behavioral patterns, and social pressures, as it is of individual competence.<sup>94</sup>



Creative activity cannot be forced. It requires a favorable climate in which creative people -- the free, unchanneled minds, the nonconformists, the individualists, the uninhibited -- may work and thrive. The frustrations, disagreements, and failures which often result from a poor climate rarely occur in an atmosphere which is tolerant of "oddball" individualists and is receptive to new ideas, which is relatively free of the pressures to produce and permits freedom to choose problems and change the direction of research, which recognizes the importance of research and the professional status of the researcher, and which rewards creativity and enhances mutual respect between scientists and management.<sup>95</sup>

The question arises as to how research management can foster an optimum climate for the development of creativity and, at the same time, give direction and speed to a research group which performs at peak effectiveness in the absence of rigid controls and the presence of freedom of inquiry?<sup>96</sup> The hallmark of an effective research leader is the recognition that one cannot "manage" and "direct" a group whose idea of a boss and of external controls is an anathema inconsistent with the basic tenets of professionalism.<sup>97</sup>

It is only in employing a leadership style which creates an atmosphere of mutual respect, integrity, and confidence that the manager may hope to provide effective direction to the research effort. He must create a climate of "research-mindedness" in which the needs, opportunities, and rewards which motivate professional people are understood and appreciated.<sup>98</sup>

A study of leadership styles in research organizations bears out this approach. Participatory leadership, characterized by a high degree of interaction and involvement with subordinates and joint decision-making practices, was found to be superior to either laissez-faire or directive leadership patterns. The results of the study confirmed that professional personnel do respond to situational factors in organizations and that the climate resulting from participatory leadership styles produces favorable motivations and attitudes.<sup>99</sup>

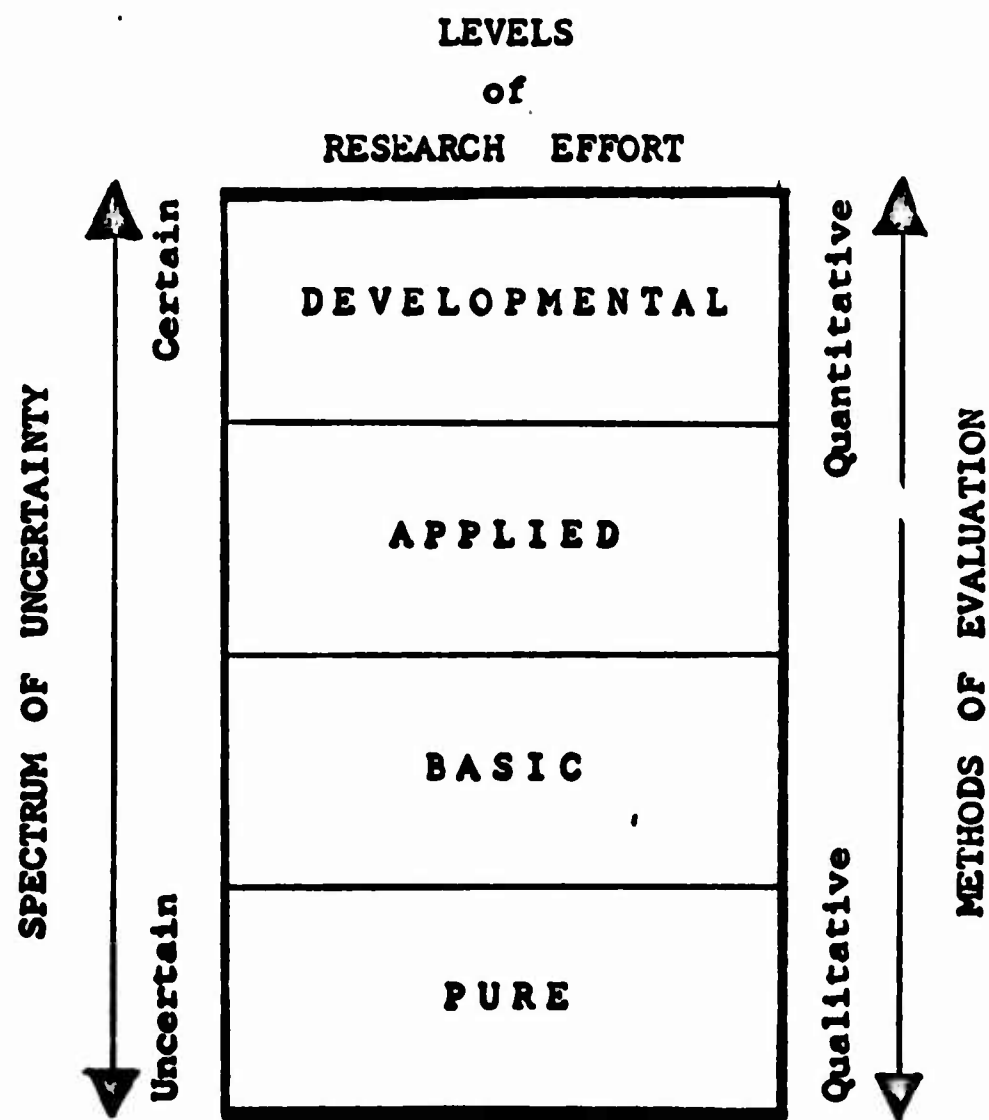
What is the significance of these findings to the establishment of methods for the evaluation of research effectiveness? Does it negate their use? The answer is found by returning to the statement made by Chris Argyris that controls, objectives, and evaluation procedures are useful and necessary, and that the less defensive an organization is, the more it can generate and accept such controls. Thus once an optimum climate for creativity and innovation is established by an understanding and sympathetic research management, then appropriate evaluation techniques may be used without the danger of decreasing the very effectiveness they attempt to measure.

#### IV. Conclusion

This report reveals that many different techniques are employed to predict and determine the results achieved from scientific endeavor. Many various means, ranging across the quantitative to qualitative spectrum of methodology, are used to appraise, measure, and evaluate research and developmental efforts. It is important to remember that no one method is a panacea -- each has its own limitations as well as its advantages. There is no one best method to be employed in analyzing research and scientific endeavor. The particular method or combination of techniques to be used is dictated by the given situation. Variables which affect the methods selected for analysis include such things as the type of research agency involved, the level of research effort, and the stage of the research life-cycle to be evaluated. A method which might be quite appropriate under one set of variables may be completely inapplicable under another set of conditions.

Whether more quantitatively-oriented methods of analysis will be employed or whether more qualitatively-determined judgments will be rendered depends, to a large degree, upon the spectrum of uncertainty -- both in relation to the levels of research effort and the stages of the developmental cycle. Therefore, the implications for evaluation, discussed in the preceding sections of this report, will be again summarized so that the important interrelationships between these variables may be more readily appreciated.

Figure 1.



**The Relationship Between  
the Levels of Research Effort,  
the Spectrum of Uncertainty,  
and the Methods of Evaluation.**

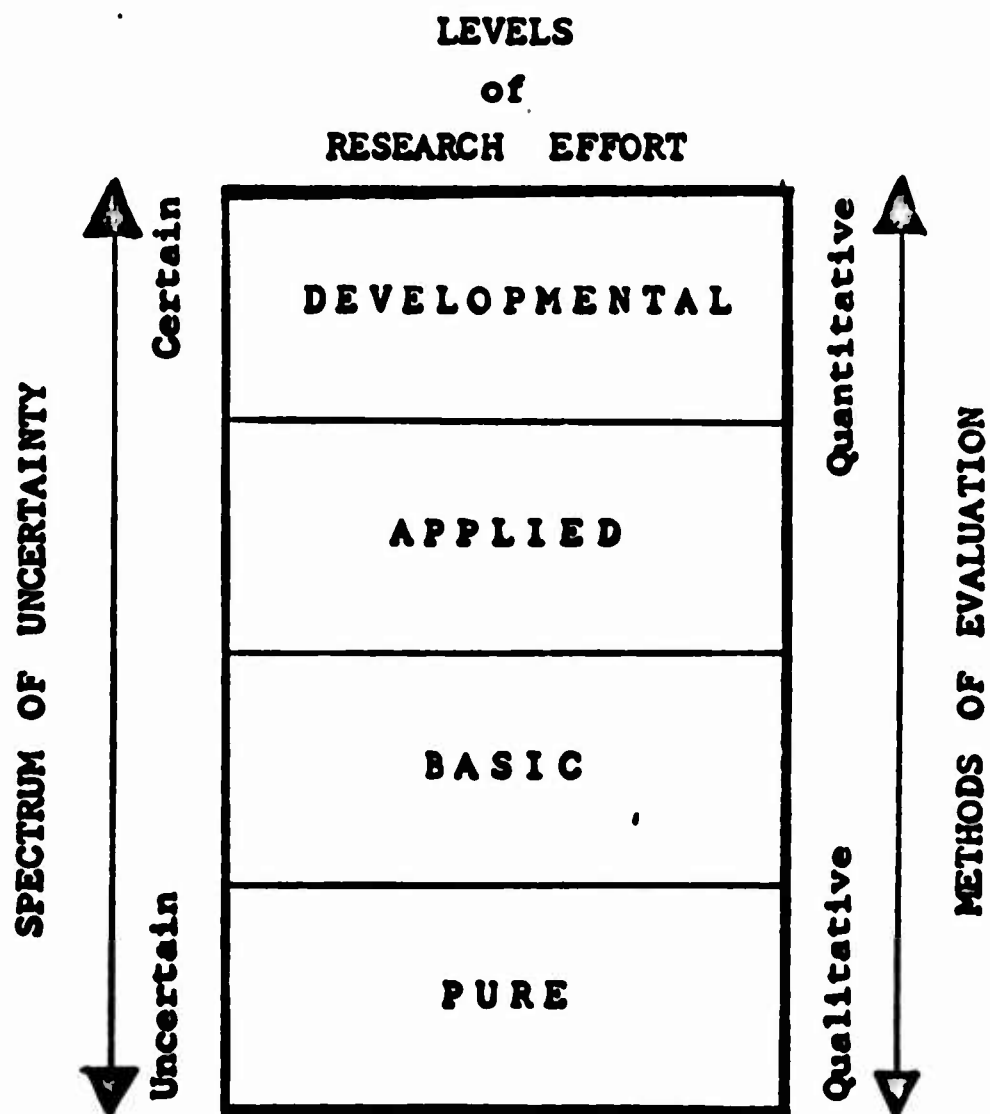
### Levels of Research Effort

It has been pointed out that the degree of uncertainty of the results of research appears to be the basic criterion for categorizing the levels of research effort. At one end of the spectrum of uncertainty is fundamental research and at the other end is developmental application. Assuming that the spectrum ranges from a great deal of uncertainty at the lower end to a fairly high degree of certainty at the upper end, then the principal implication for evaluation of research performance lies in the fact that research results may be measured with relative confidence at the upper end of the spectrum whereas the measurement of research effectiveness becomes progressively more difficult toward the lower end of the spectrum of uncertainty.

Thus, developmental research, which lies at the upper end of the spectrum and is closely concerned with producing a product whose output may be reasonably well compared to the inputs required to produce it, can be effectively evaluated by employing the more quantitative methods of analysis. However, fundamental research, which falls at the lower end of the spectrum and which is concerned with basic inquiry in its purest sense, must be evaluated with the more subjective, qualitative methods of judgment. The close relationship between the levels of research effort, the spectrum of uncertainty, and the methods of evaluation employed is illustrated in Figure 1.

It was also pointed out that research efforts are conducted by many different agencies--including the government, academic institutions, private organizations,

**Figure 1.**



**The Relationship Between  
the Levels of Research Effort,  
the Spectrum of Uncertainty,  
and the Methods of Evaluation.**

and industry. The principal implication concerning the evaluation of research effectiveness is essentially the same as that arising from the spectrum of uncertainty. The more basic the research performed by academic institutions, some government laboratories, and a few industrial enterprises, the more subjective or qualitative must be the means of evaluating these research efforts. On the other hand, the applied research and development efforts conducted by many industrial organizations as well as by some of the other types of agencies, may be gauged by the more quantitative methods of analysis. Because the methods employed by these agencies relates more to the level of research being conducted than to the type of agency they may be, no diagrammatic illustration is presented to demonstrate the relationship between these variables.

#### Stages of Development

There is also a close relationship between the spectrum of uncertainty and the stages of the developmental cycle insofar as the evaluation of R & D activities is concerned. The steps in the life-cycle of a project usually commence with the initial consideration of the proposed research, run through the various in-process research and development activities, and conclude with the completion or termination of the project in some sort of research results, whether it be a final report of research conducted or a prototype model developed through research.

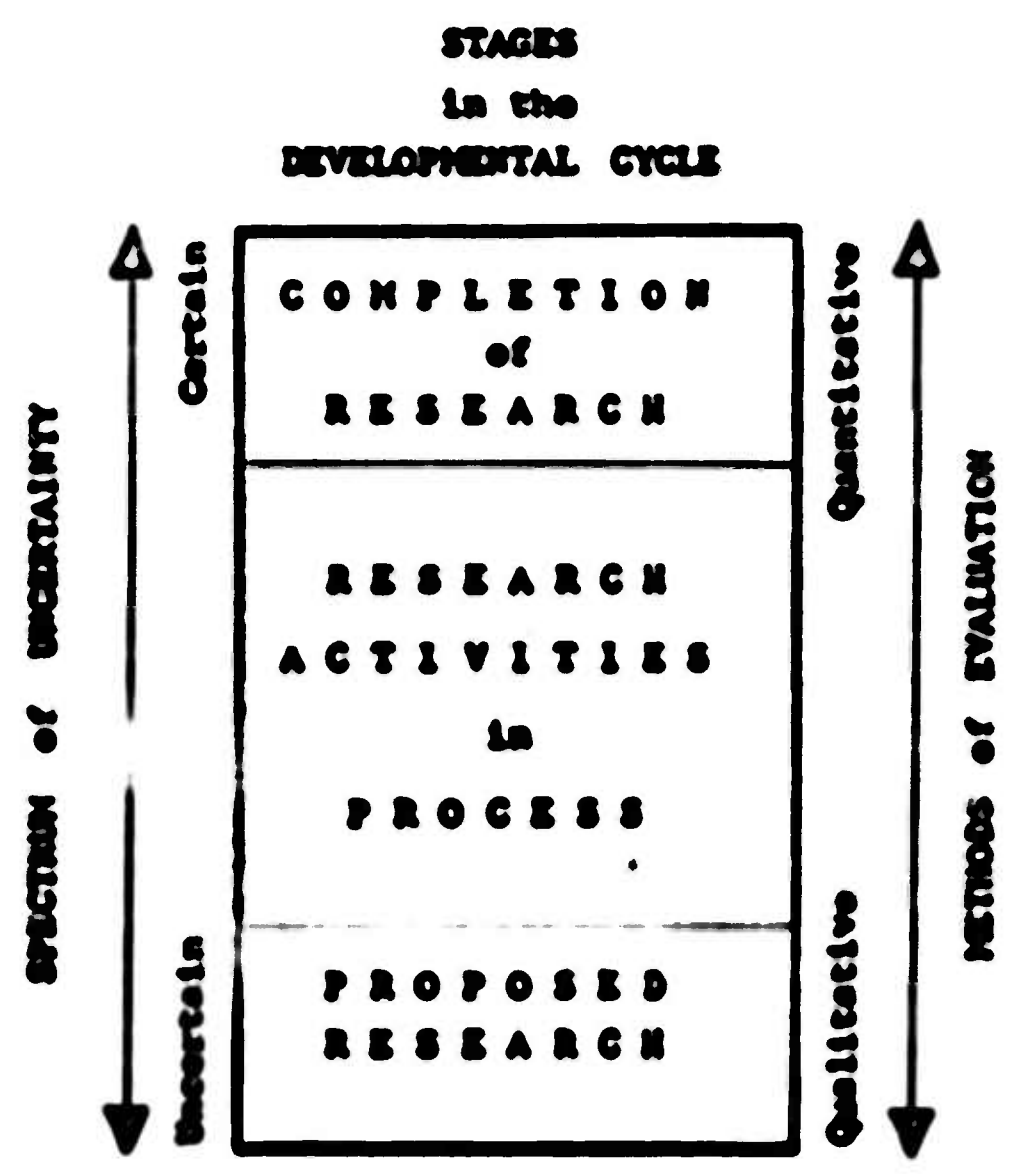


Various means of evaluation may be employed to determine how effective are the research efforts of the organization throughout the developmental cycle. Techniques for pre-appraising research are primarily concerned with establishing a set of values and criteria to assist in the evaluation and selection of alternative research proposals and against which subsequent effort and results may be compared once a project is inaugurated. Techniques for in-process measurement normally attempt to analyze the current status and degree of completion of research efforts, comparing these against criteria initially planned for the various points in the developmental cycle. And, techniques for post-evaluation of research efforts try to determine the effectiveness of research results in comparison to pre-established goals and criteria.

The methods of analysis range across the qualitative to quantitative spectrum of methodology. Generally, however, more quantitative methods of analysis may be employed for evaluating research after it has been completed or terminated, since more numerical data are normally available at that point in the developmental cycle when compared to the earlier stages of research. At the earlier, planning stages of the research, on the other hand, in pre-appraising the potential of possible research under consideration, relatively little numerical data are available and the more qualitative methods of judgment must be consequently employed. The close relationship between the stages in the developmental cycle, the spectrum of uncertainty, and the methods of evaluation is illustrated in Figure 2.

●

**Figure 2.**

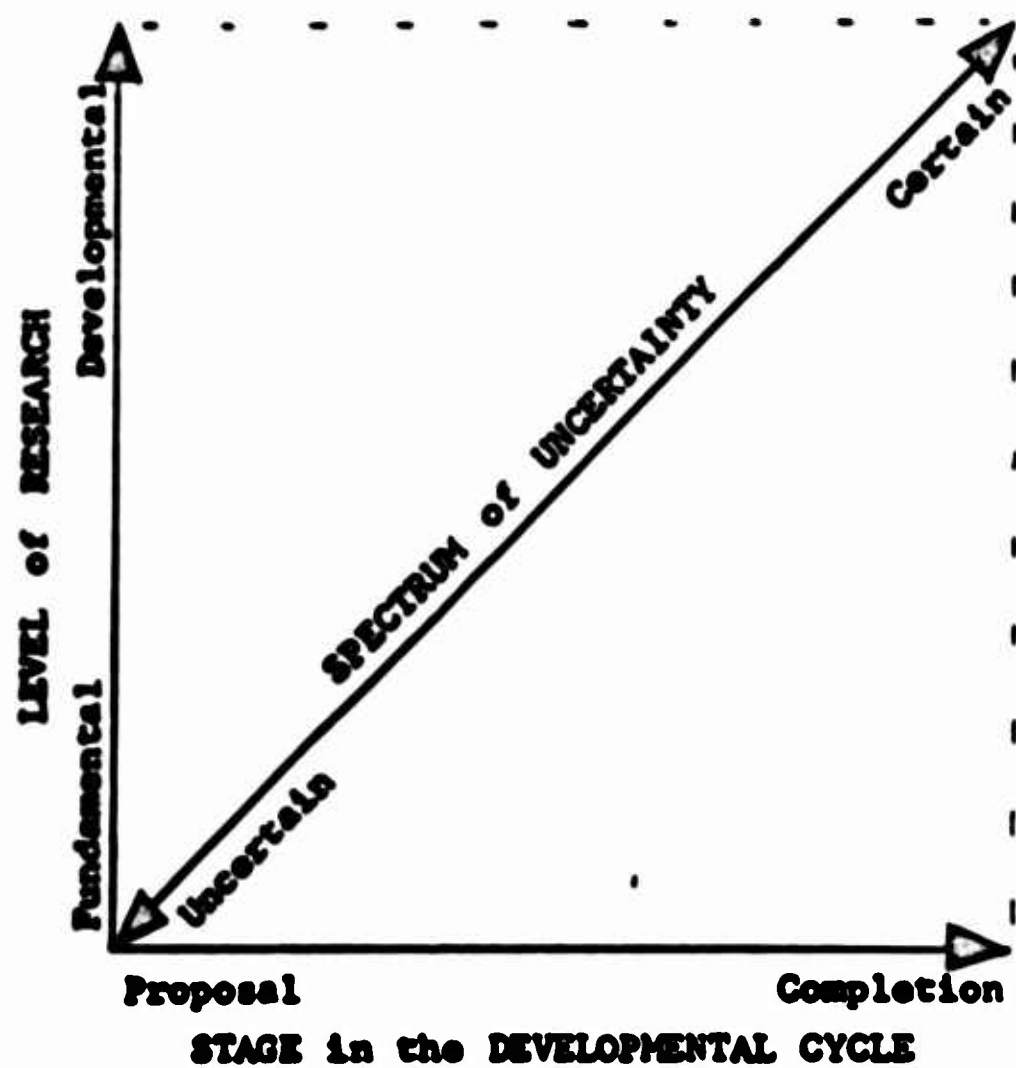


**The Relationship Between  
the Stages in the Developmental Cycle,  
the Spectrum of Uncertainty,  
and the Methods of Evaluation.**

### Interrelationships

Comparison of Figures 1 and 2 readily reveals that the level of research endeavor and the stage of the developmental cycle are both closely related to the spectrum of uncertainty. Indeed, these interrelationships might be illustrated as shown in Figure 3. The diagram is not a graph in the mathematical sense of the word, since the levels of research endeavor and the stages of development are not to be considered as the abscissa and ordinate of a graphical relationship. That is, some point on the vertical axis showing a particular level of research effort should not be viewed as correlated with some point on the horizontal axis showing a given stage in the research life-cycle, simply because respective plots drawn from these points to the line showing the spectrum of uncertainty happen to intersect at that coordinate. Instead, the diagram is presented to show that there is a relationship between some level of research effort and the degree of corresponding uncertainty, and that there is also some correspondence between some stage in the developmental cycle and the corresponding degree of uncertainty.

Since the more qualitatively-oriented methods of evaluation will normally be used at the lower end of the spectrum of uncertainty to judge the more fundamental levels of research effort and the earlier research steps in the developmental cycle, and since the more quantitatively-oriented methods will usually be used at the upper or more certain end of the spectrum to analyze the developmental levels of research and the later stages in the

**Figure 3.**

**The Relationships Between  
the Spectrum of Uncertainty and  
the Level of Research Effort or  
the Stage in the Developmental Cycle**

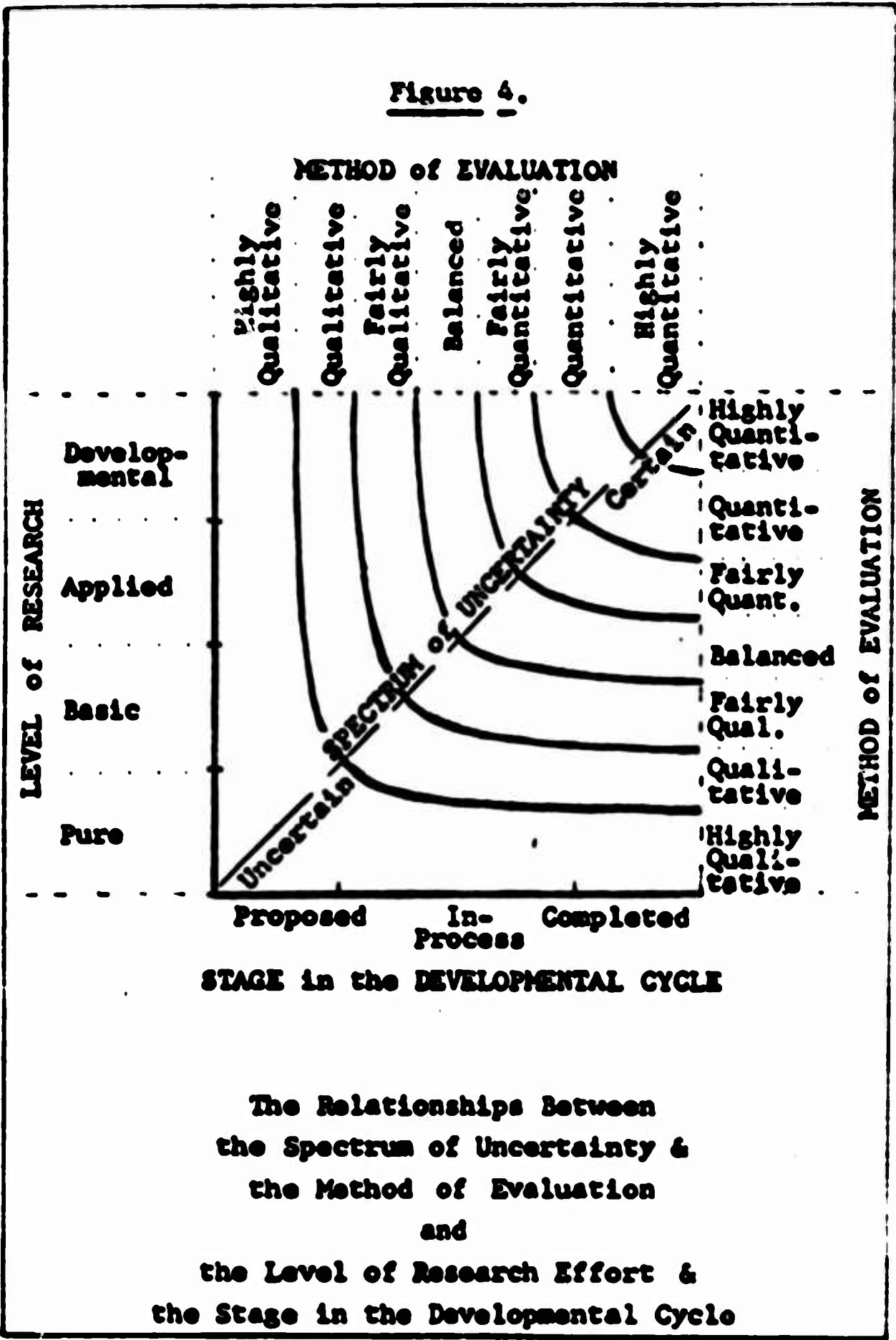
life-cycle, these relationships might be better illustrated as shown in Figure 4.

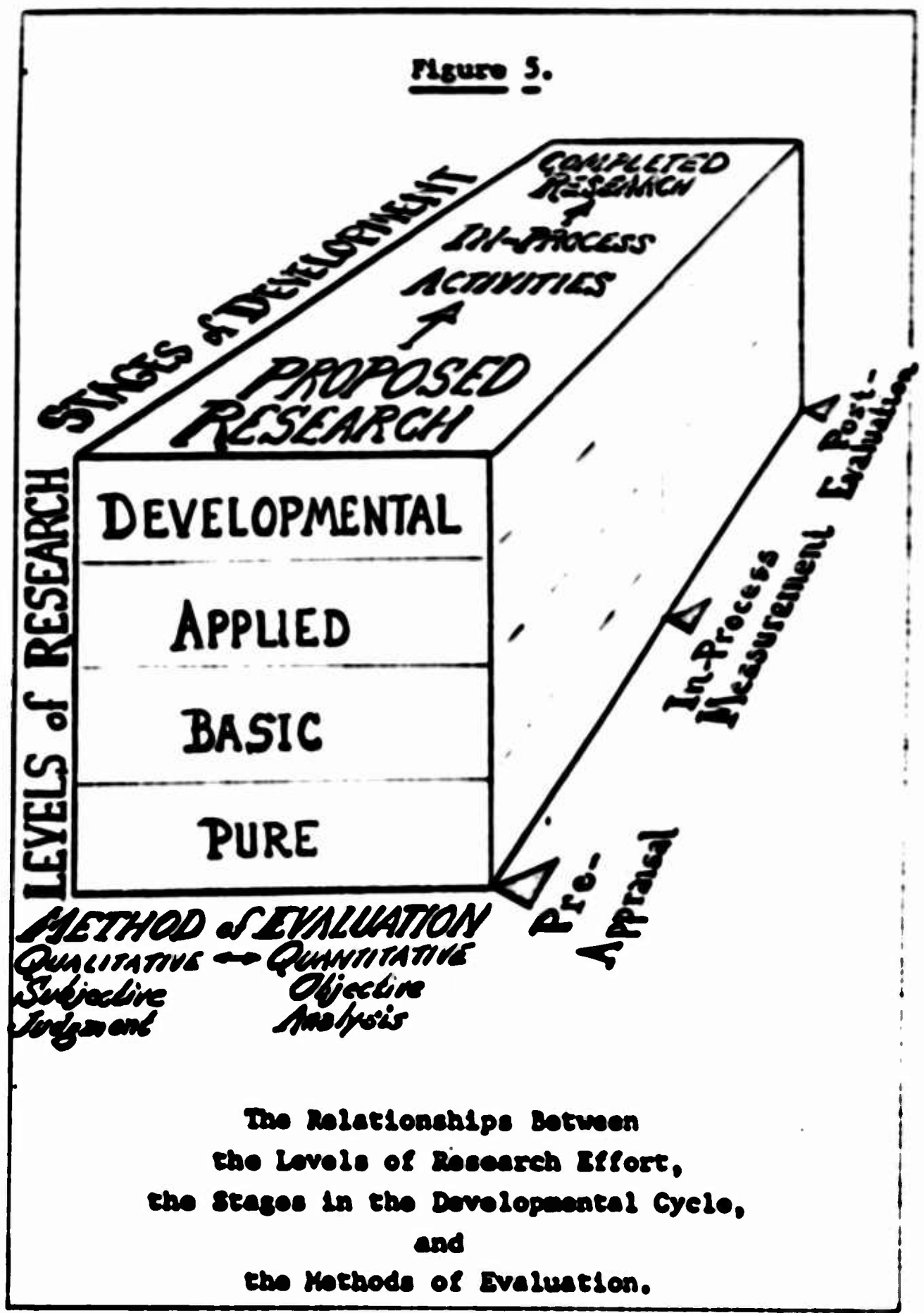
For instance, in referring to the diagram, it is noted that the developmental level of research effort near the close of the life-cycle of research activities may be analyzed with highly quantitative techniques, whereas the purer level of research in the beginning stage of the cycle must be evaluated with highly qualitative methods. The curves also reveal, for example, that mostly qualitative methods must be normally used to judge the developmental level of research in the earlier stages of the life-cycle and to evaluate the pure level of research even at the end of the developmental cycle. The diagrammatic presentation employing the "indifference" type curves for the methods of evaluation is particularly appropriate in that the difficulty in using highly quantitative techniques of measurement in almost all situations is clearly illustrated.

Still another way of looking at the relationships between the levels of research, the developmental cycle, and the methods of analysis is shown in Figure 5, which presents a three-dimensional diagram of the nature of these interrelationships. Many of the methods and techniques employed in measuring and evaluating research endeavor and developmental activities were classified within this paper in relation to their use for pre-appraisal, in-process measurement, and post-evaluation of R & D efforts.

If cross-sectional views of the diagram in Figure 5 were made at selected intervals along the dimension relating to the stage of development, at the point of pre-appraisal, at some point of in-process measurement, and at the point of post-evaluation of the research, the

Figure 4.





diagrams would reveal the general proportion of quantitative vis-a-vis qualitative methods of evaluation that might be employed for the various levels of research effort. Figures 6A, 6B, and 6C illustrate these cross-sectional views. For instance, in Figure 6A relating to the pre-appraisal of research endeavor, mostly qualitative methods of evaluation are employed not only for all the pure level of research but also for a great deal of the developmental level of effort as well. In post-evaluation of research endeavor, shown in Figure 6C, greater use might be made of quantitative measures, especially in evaluating the developmental and applied levels of research. However, as the diagram shows, the majority of the pure level of research endeavor would still be evaluated by qualitative methods even after the research has been completed.

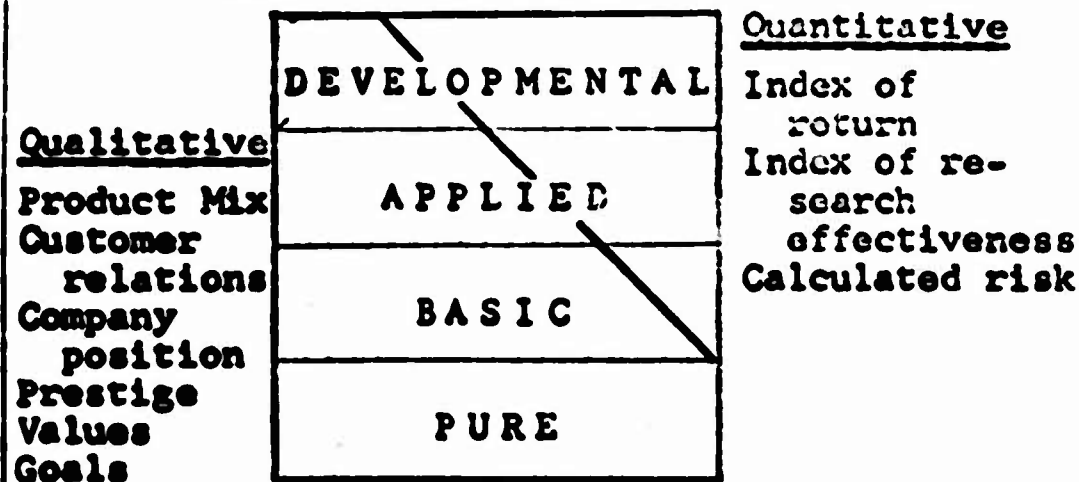
Thus, the diagrammatic presentations in Figures 6A, B, and C, closely correspond to the "indifference curve" type of approach shown in Figure 4. All these figures serve to emphasize the relatively restricted applicability of the more quantitatively-oriented methods of analysis in evaluating research and scientific endeavors.

#### Concluding Remarks

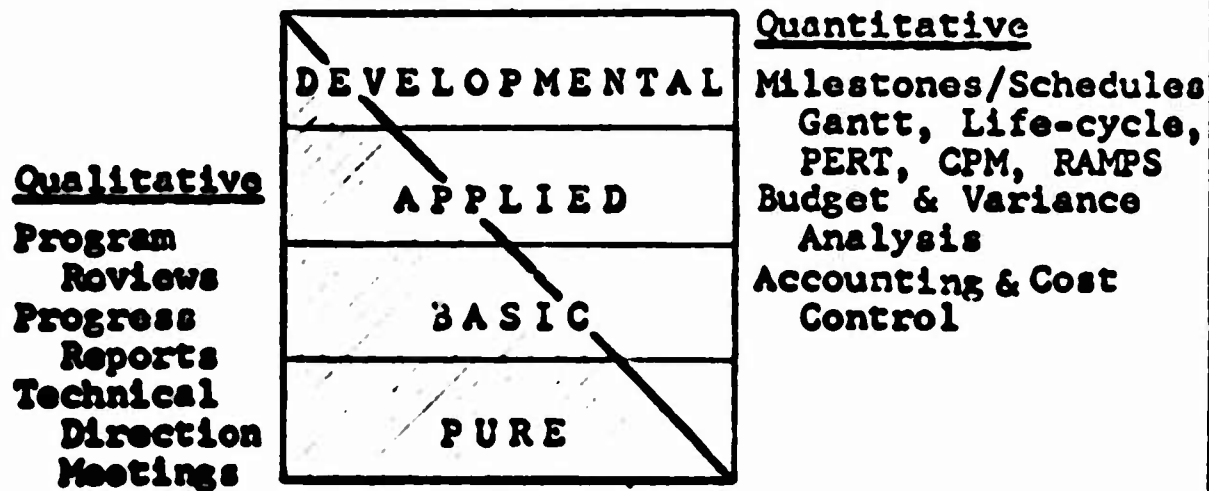
Generally, then, the following overall conclusions may be drawn about the methods of evaluating research efforts and developmental activities. First, the quantitative methods are better used in the applied and developmental levels of research, but their use in evaluating pure and basic research is of questionable value.



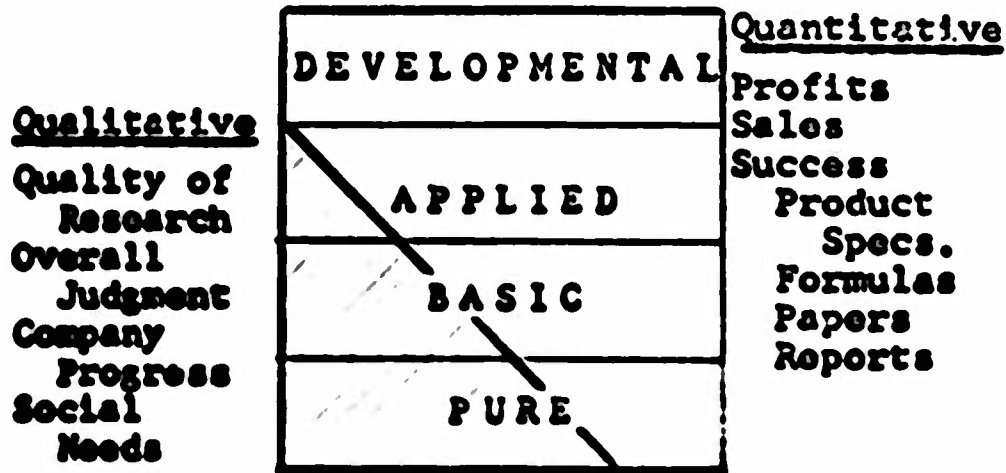
**Fig. 6A. PRE-APPRAISAL**



**Fig. 6B. IN-PROCESS**



**Fig. 6C. POST-EVALUATION**



**Cross-Sectional Views at Selected Points in the  
Developmental Life - Cycle Showing  
the Methods of Evaluation in Relation to  
the Levels of Research Effort**

They are also normally of greater value in the post-evaluation stage of research efforts than in the pre-appraisal stage of the cycle where less data are available and greater uncertainty exists. The qualitative methods of evaluation are better suited for use at the fundamental levels of research and may also be of greater value than the more quantitative techniques in the pre-appraisal stages of the developmental cycle. They can also be used to advantage in measuring the quality of research results, in estimating the intangible products of research such as papers and publications, and in evaluating the non-profit motivated types of research such as that conducted by government laboratories and fundamental research organizations.

Restated once more, the quantitative vis-a-vis the qualitative methods are at the opposite ends of the measurement spectrum and are closely related to the spectrum of uncertainty. Variations of one or the other methods or combinations of the two are, therefore, generally employed, depending upon the level of research effort and the stage of the developmental cycle, as well as many other situational factors. As one leading econometrician, Ira Horowitz, noted--after making a comprehensive review and critique of methods used for measuring the economic impact of R & D efforts--the degree of success of a research program is not to be judged solely on the extent the program achieves or surpasses its own previously established objectives, but rather in terms of the multiplicity of objectives of the firm. Horowitz stated:<sup>100</sup>

Depending upon the nature of the stated objectives, the analysis of effectiveness may require both quantitative and qualitative judgments, and these judgments will necessarily be

tempered by the knowledge that the extent to which the objectives have been attained is a function, not just of the effectiveness of the research program, but also of the effectiveness of all the other components that make up that complex organization, the economic unit.

One final point requires especial reemphasis. As has been stated by Edward B. Roberts in his book, The Dynamics of Research and Development, the human element is one of the most important variables in research and development systems.<sup>101</sup>

. . . research and development progress, measurement, and control are all strongly dependent upon the human variable. Not only is R and D productivity subject to motivation influences, but the subjective evaluations that rest at the basis of any R and D control system are also affected. Many problems in R and D may come largely from the McGregor "Theory X" difficulty, that is, the attempt to exert external control and authority on research and development without incentives that will motivate R and D people to strive toward the same ends.

Thus, it is necessary to establish an optimum climate that fosters creativity and innovation with a minimum of suspicion and distrust and a maximum of mutual respect and integrity before evaluation and measurement methods may be successfully employed.

In conclusion, because of its basic nature, scientific productivity is extremely difficult to measure. But by using quantitative means, wherever they may be appropriately employed, to assist in improving the quality and validity of subjective or qualitative judgments, the effectiveness of scientific performance may be evaluated so as to provide management with the guidance required for the direction and control of research and development efforts.

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Appendix

VARIOUS EQUATIONS CITED IN THE TEXT  
FOR APPRAISING AND EVALUATING R & D

Index of Return Formula - devised by Fred Olsen of Olin Industries, to assist in determining the potential value of a proposed program:

$$IR = \frac{EVR \times I \times ECS}{ECR}$$

where:

- IR = Index of Return; which determines whether a project is worthwhile or should be abandoned.
- EVR = Estimated Value of Research, if it succeeds; the summation of the full value of savings for one year for a process improvement plus 2 percent of the sales value for two years of a product improvement plus 3 percent of the sales value for five years for a new product.
- ECS = Estimated Chance (or probability) of Success.
- ECR = Estimated Cost of Research.

Experience has shown that the index of return should exceed the value of 3 before the project is considered worthwhile and the project should be abandoned should the index fall below 3. The percentages and time periods for the EVR term are arbitrarily assigned and have been changed in the years since it was first applied.

Miller Variation of Olin's Index - suggested by Theodore Miller; uses as a numerator the product of the following:

- (Percentage chance of technical success) X
- (Percentage chance of commercial success) X
- (Estimated unit sales per year) X
- (Profit per unit) X
- (Square root of the life of the product in years)

The formula yields a "product number" which is used as a basis for comparing various research proposals.

Figures of Merit - proposed by I. M. Ansoff as a framework for evaluation of applied research; uses two formulas based on criteria related to potential earnings and criteria related to investment. By assigning arbitrary scales to the individual criteria, both figures of profit merit and of risk merit may be computed for each project and then the competing projects may be compared. The formulas for figures of profit merit and of risk merit are as follows:



$$FM_p = \frac{(M_t + M_b) \times E \times P_o \times P_p}{C_d \times J} \times S$$

$$FM_r = \frac{C_{ar}}{FM_p}$$

where:

- $FM_p$  = Figure of profit merit.
- $FM_r$  = Figure of risk merit.
- $M_t$  = Technological merit.
- $M_b$  = Business merit.
- $E$  = Estimate of total sales (earnings) over lifetime.
- $P_o$  = Probability of success of project.
- $P_p$  = Probability of successful market penetration.
- $S$  = Strategic fit of proposed project with other projects, products, and markets of the company.
- $C_d$  = Direct investment =  $(C_{ar} + C_{pr} + C_{pd} + C_{pe} + MC + F)$
- $C_{ar}$  = Total cost of applied research effort.
- $C_{pr}$  = Total cost of product research effort (exclusive of applied research).
- $C_{pd}$  = Total cost of product development effort.
- $C_{pe}$  = Total cost of product engineering effort.
- $MC$  = Working capital required.
- $F$  = Total cost of extra facilities required, such as staff, buildings, etc.
- $J$  = Joint cost effect; savings factor in direct investment resulting from use or sharing of existing facilities and capabilities.

**Index of Research Effectiveness** - described by G. K. Teal of Texas Instruments; used to quantitatively evaluate research projects, the index is the product of four other indices:

$$\text{Index of Return on R \& D} = N / 25 S$$

where:

- $N$  = Net profit during the life of the resulting product.
- $S$  = R & D costs.
- $25$  = Figure taken from statistics on return on R&D collected by the National Science Foundation.

$$\text{Index of Return on Assets} = N / 13.5\% A$$

where:

- $N$  = Net profit during the life of the resulting product.
- $A$  = Assets which were required.
- $13.5\%$  = Figure based upon a judgment that a worthwhile product should make possible a favorable return of 13.5%.

$$\text{Index of Dollar Volume} = b / (B/25)$$

where:

- b = Billings made possible by the product.
- B = Total billings during the product's lifetime.
- 25 = Figure based upon a judgment that a worthwhile product should make possible a 45% increase in total annual billings.

$$\text{Index of Market Capture} = b / (N/2)$$

where:

- b = Billings made possible by the product.
- N = Total available market.
- 2 = Figure based upon a judgment that a worthwhile product should capture 1/2 of the total market.

If, when multiplying these four indices to obtain an index of research effectiveness, a factor of one or more is obtained, the project is considered a worthwhile investment.

Allowable Research Investment Formula - described by Ralph Marley of General Mills; used as a guide to the amount of money that a company can afford to spend on research leading to a new development:

$$R = 2 (0.01 RNS - P - W)$$

where:

- R = Research and development costs before taxes.
- P = Plant investment.
- W = Working capital required.
- Y = Recoupment period (period in which costs must be recovered).
- S = Annual sales volume of new product.
- N = Minimum acceptable net profit on sales.

Moravitz Functions - suggested by Ira Moravitz for anticipating returns from prospective ventures. If the time pattern of returns and expenditures can be approximated by a function of the form:

$$R(t) = K t^a e^{-t/p}$$

then

$$\int R(t) e^{-rt}$$

gives the cumulated research costs discounted at a rate  $r$  per unit of time. Dividing this expression by a similar expression for profits would give a rate of return for prospective ventures.



**Calculated Risk Basis Formula** - used to compute a project's rate of return to be measured against a standard which states an acceptable return:

$$R = \frac{\sum_{i=0}^n [P(I_i)]}{\sum_{i=0}^n (O_i)}$$

where:

- R = The project's rate of return on total funds invested.
- $I_i$  = Net estimated income from the project's results in any  $i$ th year.
- n = The last year in which incomes are expected.
- i = The year hence in which the income or outlay will occur.
- P = Probability of receiving the  $i$ th income.
- $O_i$  = The net incremental investment outlay in any  $i$ th year.

**Unadjusted Return Technique** - makes broad calculations of a project's potential return versus its costs. Projects are more desirable as R increases, the minimum R required to accept any project being 30 percent before taxes:

$$R = \frac{\sum_{i=0}^n (I_i)}{\sum_{i=0}^n (O_i)}$$

where:

- R = The rate of return on investments in the project.
- $I_i$  = The income the project will yield in each  $i$ th year.
- $O_i$  = The incremental outlay required in each  $i$ th year.
- i = The number of years hence each income or outlay is realized.
- n = The number of years hence the last income or outlay is realized.

**Hoskold Transformation** - computes the present worth of the incomes the project will yield for comparison with the present value of the project's actual and projected costs in order to determine if the project is financially acceptable:

$$P = \frac{D}{R + \frac{R'}{(1 + R')^n}}$$

where:

- P = The present worth of the incomes the project will yield if successful.  
 D = The average annual incremental income yielded if the project is successful.  
 R' = The average net return on capital invested in the enterprise.  
 R = The current rate of interest on investments.  
 n = The number of years within which research costs must be recovered.

Present Value Techniques - often are used since the  $(1 + R)^{-1}$  factor, shown below, is readily available in standard financial tables and projects can be ranked in order of ascending P to determine their relative desirability:

$$P = \sum_{i=0}^n [C_i (1 + R)^{-1}]$$

where:

- P = The project's present value.  
 C<sub>i</sub> = The net cash inflow in each *i*th year (net cash outflow in a year would make C<sub>i</sub> negative).  
 i = The number of years hence the cash flow occurs.  
 n = The number of years hence the last significant cash flow occurs.  
 R = A discount rate which measures the rate at which the company would invest its money to obtain cash flows of equal risk in the same year.

Life-Cycle Equation - cited by Raymond Villers. Once the pattern of the first cycle of development has been determined, the pattern for all the subsequent cycles may be determined by the following equations:

$$y = k (1 - e^{-at^2})$$

$$y' = 2 k a t e^{-at^2}$$

where:

- y = Manpower to date, in man-months.  
 y' = Manpower for each month.  
 k = Total effort for the cycle in man-months.

- a = Parameter governing the shape of the curve.
- t = Time from start of the cycle, in months.
- e = A constant, the base of natural logarithms = 2.71828...

**Variance Analysis Formulae** - cited by Raymond Villers. Two formulae are used to evaluate the expected cost in time and money of pursuing an alternative solution to a problem. After the optimum alternative is selected the formulae summarize the situation and show how technical progress is related to actual expenditures of time and money. The formula for the time factor is:

$$V_t = O_t - (A_t - E_t)$$

where:

- $V_t$  = The predicted time variance.
- $O_t$  = Scheduled time for the project.
- $A_t$  = Time already spent on the project.
- $E_t$  = Amount of time that, on the basis of the revised schedule, will be needed to complete the project.

By adding  $V_t$  to the expected completion date, one can show the revised estimated completion date. This revised estimate must be compared to the company's commitments and to the needs of the market before it can be definitely considered as acceptable. The second formula translates the first into dollars:

$$V_b = O_b - (A_b - E_b)$$

where:

- $V_b$  = The predicted variance from the budget.
- $O_b$  = The original budget for the project.
- $A_b$  = The actual expenditures since the start of the project.
- $E_b$  = The estimate cost of  $E_t$ .

The variance  $V_b$  may be due to the fact that  $A_t$  exceeds the original schedule. It may also be due to the fact that research people have reported that  $E_t$  has to be revised. In any case, the variance  $V_b$  provides the information needed and will be included in the reports issued at the various levels of management.

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